Fred: Horne.
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Compliments

AN

EXPERIMENTAL INQUIRY

INTO THE

PHYSIOLOGY

OF

CUTANEOUS ABSORPTION,

AND ITS

APPLICATION TO THERAPEUTICS;

BEING

THE ESSAY TO WHICH THE MEDICAL FACULTY OF THE UNIVERSITY OF EDINBURGH AWARDED A GOLD MEDAL, AT THE GRADUATION, ON THE 1st AUGUST 1837.

BY

WILLIAM HERRIES MADDEN, M.D.,

LICENTIATE OF THE ROYAL COLLEGE OF SURGEONS,
EXTRAORDINARY MEMBER, AND LATE SENIOR PRESIDENT, OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH.

Felix! Qui dicat—"Ευρηκα."

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TO

W. SHARPEY, M.D., F.R.S.E.,

PROFESSOR OF ANATOMY AND PHYSIOLOGY IN UNIVERSITY COLLEGE, LONDON,

WHOSE VARIED TALENTS, PROFOUND KNOWLEDGE,

AND ENTHUSIASTIC ZEAL IN PHYSIOLOGICAL RESEARCHES,

CAN BE FULLY APPRECIATED

BY THOSE ONLY WHO HAVE RECEIVED THE BENEFIT OF HIS

INVALUABLE INSTRUCTIONS,

THIS ESSAY

IS, BY PERMISSION, MOST RESPECTFULLY DEDICATED,

AS A SLIGHT TOKEN

OF ESTEEM AND GRATITUDE,

BY HIS FRIEND, AND FORMER PUPIL,

THE AUTHOR.

TO

CHARLES CHADWICK, M.D.,

EXTRAORDINARY MEMBER OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH,

THIS ESSAY

IS ALSO MOST AFFECTIONATELY INSCRIBED,

AS A SLIGHT REMEMBRANCE

OF OUR HAPPY HOURS OF FAMILIAR INTERCOURSE,

AND A PLEDGE

OF THE UNFADING ATTACHMENT OF HIS FRIEND,

THE AUTHOR.

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INTRODUCTION.

The extensive relations which exist between the skin and the rest of the animal frame, its wide range of sympathies, the number and importance of its own peculiar diseases, and the influence which it exerts over the disorders of internal organs, combine to render the investigation of its structure and functions no less interesting to the physiologist than important to the practical physician.

The wide field thus laid open for inspection has not been suffered to remain uncultivated. But though the labourers have been many—though among them we find the names of men dear to every lover of the science they adorned—though zeal, talent, and patience have been expended in the task, it is still uncompleted. Anatomy has not yet fulfilled her allotted part, while physiology, delayed by numerous obstructions, has failed in interpreting the functions of an organ whose structure yet remained unknown.

But of all the functions attributed to the skin, that of absorption is perhaps the one involved in most obscurity. It is a melancholy but instructive task to contemplate the revolutions of opinion which the subject has already undergone; melancholy, because it demonstrates in a most convincing manner how unstable and fragile are even our best conceived theories regarding all vital actions; and instructive, because it points out the danger of hasty generalization, and warns

us to avoid this rock, on which so many have made shipwreck.

It cannot, however, be denied, that the subject is surrounded by no ordinary difficulties—difficulties involved in the very nature of the inquiry, and to overcome which demands a far greater share of skill, of patience, and of perseverance, than any single observer can hope to possess. It is only, therefore, by carefully marking the errors committed by his predecessors in the inquiry—by rigidly correcting their inaccuracies, and attentively observing the facts brought before his notice, that the students of this department of medical science can have any reasonable expectation of success.

Such has been my aim in the following pages, which contain all the information I have been able to collect. How far these efforts have succeeded, it is not for me to judge. I approached the subject with a full conviction of its difficulties and my own incapacity; and all that I would here claim, is the merit of impartiality. No fact, no argument has been omitted, because it seemed to militate against the view which a careful study of the question has induced me to adopt. I have given each a full, and, I trust, a fair consideration, at the same time that I have endeavoured to refute the arguments, and prove how insufficient they are to determine the point in question.

From a conviction that the first and most essential step towards obtaining a correct acquaintance with the functions of any part, is a perfect knowledge of its structure; and having, in the course of my inquiries, had frequent occasion to remark the exceedingly discordant opinions entertained by different writers upon the structure of the cutaneous textures, and particularly of the epidermis, I have premised a brief sketch of the anatomy of the skin, before proceeding to the more immediate subject of the essay. And with a view to prevent confusion in the succeeding parts, I have preferred introducing into these preliminary remarks a few observations upon the history of our subject.

We learn from the works of the earlier classical authors, that a belief was prevalent at their time in the efficacy of poisons when applied to the surface. Such we are informed were the means employed by Medea to revenge herself upon her successful rival, whose horrid tortures, induced by the fatal present, are thus graphically described by the poet :—[1]

ὄυτ' ὀμμάτων γὰρ δῆλος ἦν κατάστασις,
ὄυτ' εὐφυὲς πρόσωπον, ὅιμα δ'ἐξ ἄκρου
ἔσταζε κρατὸς, συμπεφυρμένον πυρὶ,
σάρκες δ'ἀπ' ὀστέων, ὥστε πεύκινον δάκρυ,
γναθμοῖς ἀδήλοις φαρμάκων ἀπέρρεον.

To a similar cause is the death of Hercules attributed. The robe of Nessus, imbued with the poison of the Lernean hydra, though but intended by his jealous wife to call back her husband's wandering affections, induced a far more tragical conclusion :—[2]

προσπτύσσετο
πλευραῖσιν ἀρτίκολλος, ὥστε τέκτονος,
χιτὼν ἅπαν κατ' ἄρθρον· ἦλθε δ'ὀστέων
ἀδαγμὸς ἀντίσπαστος.

And in these agonies the hero perished. Thus also Circe, when she wished to restore to their original shapes the companions of Ulysses, whom her incantations had converted into the similitude of swine, used some external application for the purpose :—[3]

ἠδὲ δι' αὐτῶν
ἐρχομένη προσάλειφεν ἑκάστῳ φάρμακον ἄλλο,
τῶν δ'ἐκ μὲν μελέων τρίχες ἔρρεον, ἃς πρὶν ἔφυσσε
φάρμακον οὐλόμενον, τὸ σφιν πόρε πότνια κίρκη·
ἄνδρες δ' αἶψ' ἐγένοντο νεώτεροι, ἢ πάρος ἦσαν,
καὶ πολὺ καλλίονες, καὶ μείζονες εἰσοράασθαι.

[1] *Euripides.* Medea, l. 1187. [2] *Sophocles.* Trachiniæ, l. 769.
[3] *Homer.* Odyssey, L. x. 391.

Duval relates many singular instances of the same belief, often indeed heightened by the grossest superstitions.[1] Of this nature are the wonderful accounts of plants, the sole touch of which could at once remove all danger from the bites of venomous animals; of that root of peony, which preserves from epilepsy those who carry it; and of the horse chesnut, which, carried in the pocket, prevents or cures the severest hæmorrhoidal affections.

But passing by all these, which in truth are more curious than instructive, let us inquire how far the practice of the older physicians will warrant the conclusion, that they attributed this function to the skin. It appears that Herodicus, who flourished about 443 A. C. and Prodicus, who shortly afterwards succeeded him, were the first practitioners who attempted to cure diseases by the application of medicines to the surface of the body.[2] Oil was the principal agent they employed, this remedy having been suggested by the obvious effects in the preservation of health produced by gymnastic exercises, of which the bath and unctions formed an essential part. And the practice which the ancients frequently observed, of exposing their bodies when anointed to the rays of the sun, that their skin might the more readily imbibe the nutrient fluid, is distinct evidence of their belief in cutaneous absorption.[3]

Thus Persius, Sat. iv. 17.:—

> "Quæ tibi summa boni est? Unctâ vixisse patellâ?
> Semper, et assiduo curata cuticula sole."

And Martial, L. x. Ep. 12.:—

> "I, precor, et totos avidâ cute combibe soles."

[1] *Duval.* "Sur la Médecine Eispnoïque des Anciens." Reported in Recueil Period. de la Soc. Med. de Paris, viii. 43.

[2] *Duval.* loc. cit.

[3] Vid. *Hunter* on the External Use of Oil. Ed. Med. and Surg. Journ. ii. 185.

Hippocrates was in the frequent habit of employing medicated frictions for the cure of menstrual complaints.[1]

Diocles, his successor, made great use of a mixture of ox bile and hellebore to produce vomiting.[2]

Theophrastus, the first of botanists, observed that frictions made upon the surface of the body, with aromatic preparations, determined eructations of similar odours.[3]

At the same time Diagoras, fearing the internal employment of opium, prescribed it in the forms of poultice, frictions, and plasters, and bears testimony to its efficacy when thus exhibited.[4]

Celsus applied many of his remedies externally; and the importance which he attached to this method of cure is sufficiently evident from the number and complexity of the malagmata, &c. which he recommends.[5]

In the time of Trajan, when Asclepiades and Archigenes practised at Rome, this method was in great vogue. They endeavoured to avoid every thing disagreeable to their patients, and therefore only employed baths, frictions, exercise, and external applications. Asclepiades had a preparation which he denominated "Myracopon," a mixture of aromatics and some drastic medicines. This he employed with great success in arthritic complaints and abdominal obstructions.[6]

Decoctions of poppies were much employed as an external application by Aretæus, who also applied aloes to the epigastrium in cases of abdominal disease, and prescribed inunction in various complaints.[7]

Galen and his followers were well acquainted with the fact, that medicines finely pulverized are thus carried into the circulation. He expresses himself very clearly in favour of the absorbing power of the skin, "Ὥσπερ διὰ τῶν εἰς τὸ δέρμα περαινομένων στομάτων, ἐκρίνωσι μὲν ἔξω πᾶν ὅσον ἀτμῶδες καὶ καπνῶδες περίττωμα, μεταλαμβάνουσι δὲ εἰς ἑαυτὰς, ἐκ τοῦ περιέχοντος ἡμᾶς ἀέρος, οὐκ

[1] *Duval.* loc. cit. [2] Ibid. [3] Ibid. [4] Ibid.
[5] *Celsus* de re Medicâ. l. v. [6] *Duval.* l. c. [7] Ibid.

ὀλίγην μοῖραν· καὶ τοῦτ' ἔστι τὸ πρὸς Ἱπποκράτους λεγόμενον, ὡς ἐκπνοῦν καὶ εἰσπνοῦν ἐστὶν ὅλον τὸ σῶμα."[1]

All succeeding practitioners of whom we have any account, until the destruction of the Roman empire, were in the habit of employing this as a secondary means of cure.[2]

The Arabian physicians were accustomed to apply remedies to the skin, alike for the purpose of promoting the secretions of urine or perspiration, and of exciting the action of vomiting.[3] And Avicenna recommends inunction with warm oil for the cure of convulsions, and baths of the same for tetanus.[4]

At the beginning of the eighteenth century this mode of practice came much into use, but soon again fell into disrepute, from the exaggerated statements of Privatti, Veratti, Palma, Brigoli, &c. who pretended that they could cause purgation by applying electricity to a patient, who, while seated on an insulated stool, held in his hands tubes of glass, daubed internally with purgative medicines.[5] Towards the end of the same century, the researches of Monro, Hunter, Mascagni, Cruickshank, and Hewson, upon the lymphatic system, again attracted the attention of physiologists to the neglected subject; and the inquiries of Spallanzani, Chiarenti, Brera, Alibert, Pinel, and Chrestien, which soon followed, were decidedly in favour of the existence of this function. But all these have been yet once more called in question, and the existence of cutaneous absorption totally denied, by Seguin, Currie, Klapp, and many others; while on the other hand, the researches of Edwards, Young, Dill, Westrumb, Collard, &c. are equally conclusive in its favour.

From the rapid sketch we have thus taken, it will be at once perceived, that this, in common with almost every other

[1] Vid. *Hodgkin.* Thesis de Absorbendi Functione. Appendix to *Edwards* on Life, p. 353. [2] *Duval.* l. c. [3] *Hodgkin,* l. c.
[4] Vid. *Westrumb.* Journ. Complem. xxx. 362.
[5] Vid. *Wallace.* Researches on Chlorine. Lond. 1822. Note, p. 10.

subject of medical science, has undergone more than one revolution in its progress. At present physiologists are considerably divided in opinion, the balance however preponderating somewhat to the negative scale; at least in this country, where it may be remarked much less attention has been paid to the elucidation of the subject than its interest and importance would appear to demand.

ON

CUTANEOUS ABSORPTION.

DIVISION I.

CHAPTER I.

COMPARATIVE ANATOMY OF THE SKIN.

Few textures of the body present more striking varieties in form and outward appearance, than the tegumentary organs in the different classes of the animal creation; and yet upon a more attentive examination, it will be seen that the majority are constructed upon the same plan, only modified to suit the peculiar condition of the creature. Thus, in the lowest tribes, as the polygastrica, porifera, zoophyta, and acalepha, the simple cellular tissue of which their whole bodies are composed, does not appear to be covered by any distinct or separate membrane, being merely somewhat condensed externally, and exuding a protecting mucus.[1]

In the echinodermata, the integuments in a great measure

[1] *Grant.* Lectures in Lancet, 1833-4, p 997.

constitute the parietes of the body: in some consisting of a thick coriaceous membrane, in which masses of calcareous matter are imbedded, and a thinner external envelope or epidermis; in others, of a white fibrous layer, or true skin, a soft coloured layer, corresponding to the so-called "rete mucosum," and a cuticle.[1]

In the majority of entozoa, the cutaneous organs consist of a cuticle and cutis.[2]

The researches of Professor Ehrenberg have shewn that the infusoria, instead of being, as was formerly imagined, composed of a simple homogeneous mass, are possessed of a highly organised structure. Accordingly, the envelope of the body in the hydatina consists of a double transparent membrane,[3] while in others it is still more complicated, the body being protected by a covering or lorica, of which there are several varieties. Sometimes it resembles the shell of a tortoise, surrounding the animal completely; sometimes it covers the back only; in other cases it exists in the form of a bell or cylinder, closed at one end; and again in others it is bivalved. Some individuals are also provided with an extremely curious organ, viz, the lacerna or mantle. This is a sort of gelatinous covering, which would seem to be formed out of the most external layer of the body; within it the substance of the animal separates into young ones, which after a time burst its parietes and escape, leaving the parent a mere bag.

The integuments of the cirrhopoda consist of a mantle, or true skin, between which and the epidermis a variable number of shelly plates are inserted.[5]

In the annelida, again, the skin returns to a more simple

[1] *Sharpey.* Cyclop. of Anat. and Phys.—"Echinodermata."

[2] *Rhind.* On Intestinal Worms, 8vo. Lond. 1829.

[3] *Gairdner.* Account of Ehrenberg's observations in Edin. New Phil. Jour., 1831.

[4] *Sharpey.* Further account of ditto, 1833.

[5] *Coldstream.* Cyclop. Anat. and Phys.—"Cirrhopoda."

type, consisting solely of a soft mucous-looking dermis, in which no distinction of parts can be observed, and which is not covered by any epidermis.[1]

The higher orders of articulata possess more complex tegumentary organs. According to Cuvier, the insecta, in every stage of their existence, have a true external epidermis. The colours observable in the larvæ are dependent upon a mucous substance, interposed between the epidermis and muscles, and considered by him as analogous to the rete mucosum of vertebrate animals. To this substance, dried and mixed with a horny matter, (now denominated chitine), he attributes also the colours of the imago, or perfect insect. Cuvier does not describe any true skin, but more recent researches have shewn that beneath the horny covering there exists a whitish fibrous membrane, sometimes indeed very thin, but often divisible into several layers, and which would seem to perform the offices of a dermis.[2]

The arachnida, as possessing precisely similar integuments, require no further notice.

In some crustacea, the skin always continues of a soft consistence, but in the majority it acquires great hardness, forming a solid case in which all the soft parts are included. It consists essentially of three parts,—a corium, an epidermis, and a pigment. The corium is a thick, spongy, and very vascular membrane, which secretes the epidermis; in the interior of this the calcareous matter is deposited, while the outermost layer is mixed with the pigment, an amorphous substance, which is probably also secreted from the dermis.[3]

The mollusca generally possess a true skin, and an epidermis, which covers the shells, where they exist, as well as the free surface of the body.[4] There would be little advantage in describing particularly the integuments of each separate

[1] *De Blainville.* Dict. des Sc. Nat.—"Vers."
[2] *Kirby and Spence.* Introduction to Entomology, iii. 101.
[3] *H. M. Edwards.* Cyc. Anat. and Phys.—"Crustacea."
[4] *Grant.* Loc. cit.

order, inasmuch as they are perfectly analogous in structure; we pass on, therefore, to the consideration of these organs, as they exist in the vertebrata.

In fishes, the corium is thick, soft, gelatinous, and highly sensitive, intimately attached to the subjacent muscles, and especially strong where the superficial scales are thin or wanting.[1] The cuticle, in the interval between the scales, of which it forms the entire substance, resembles a thin mucous layer.[2]

Much difference of opinion exists regarding the skin of the amphibia, which, as being of considerable importance in our present inquiry, demands particular attention. According to Magendie[3] and Hollard,[4] the integuments in these animals differ essentially from those of all other vertebrata, in not possessing a cuticle, the surface of the true skin being merely covered with a layer of mucus; and they therefore consider this organ as perfectly analogous to the mucous membrane lining the internal cavities. On the other hand, the researches of Grant[5] and Bell[6] tend to shew that no such difference exists, that these creatures do not present an exception to the general rule, and that the epidermis, though thin, is perfectly distinct. Indeed the fact of the repeated shedding of a membranous-looking layer, from the surface of the skin, which is allowed by all inquirers, even Magendie and Hollard themselves, would, I apprehend, be alone sufficient to settle the question, as being totally different from any phenomenon observable in ordinary mucous membranes; but all doubt is at once removed, by a reference to what occurs in toads. "In the toads," says Mr. Bell, "a very curious process takes

[1] *Grant*, l. c.

[2] *Breschet.* Nouvelles Recherches, &c., 8vo. Paris, 1835.

[3] *Magendie.* Leçons sur les phénomenes physiques de la vie, 8vo. Paris, 1836, p. 85.

[4] *Hollard.* Precis d'Anatomie Comparée, 8vo. Paris, 1835, p. 255, &c.

[5] *Grant*, l. c.

[6] *Bell.* Cyclop. Anat. and Phys.—"Amphibia."

place for its removal. When the cutiele has become dry and unyielding, and a new and softer surfaee is required, the deeiduous layer splits down the median line of the back and of the abdomen at the same time. The whole cutiele is thus divided into two parts. By numerous convulsive twitchings and contortions of the body and legs, this separation beeomes more and more considerable, and the cutiele is gradually brought off the baek and belly, in folds towards the sides. It is then loosened from the hinder legs, by similar movements of those limbs, and finally removed from them by the animal bringing first one, and then the other, forwards under the arm, and by then withdrawing the hinder leg, its cutiele is left under the foreleg. The two portions are now pushed forwards to the mouth, by the help of which, the anterior extremities are also divested of it. The whole mass is now pushed by the hands, into the mouth, and swallowed at a single gulp. The new cutiele is bright, soft, and covered with a colourless mucus; the old skin was harsh, dry, dirty, and opaque." What more perfect evidence of the existence of this membrane can be required? And if these animals possess it, can it be supposed that the others of the same class are destitute of a similar covering?

The different parts of the skin are very distinct in reptilia, the epidermis being often very thick.[1]

In birds, the corium is very thin, adhering to the subcutaneous muscles by cellular membrane; the rete-mucosum rarely contains any colouring matter where the feathers grow, but in the naked parts of the integuments it frequently exhibits the richest colours; the epidermis is thin, dry, and scaly, probably from the high temperature of the surface.[2]

The class mammalia exhibit the cutaneous organs in their greatest perfection; but for the present we delay the consideration of their structure, since all that is necessary will be most

[1] *Hollard*, loc. cit. [2] *Owen*. Cyclop. Anat. and Phys.—"Aves."

conveniently introduced while we are reviewing the system as it exists in man.

It may appear to some that these remarks are out of place, in an essay essentially devoted to the consideration of one particular function of the skin; but a moment's reflection will, I think, render it apparent, that the sketch we have just taken is by no means unimportant. If it be true, that analogy of structure renders analogy of function, to say the least, extremely probable; then must the striking similarity which we have seen to exist in these organs, throughout the animal kingdom, be a powerful collateral evidence in favour of Cutaneous Absorption, since even its most determined opponents are compelled to admit that its existence in the lower tribes is unquestionable.

CHAPTER II.

OF THE DERMIS.

The dermis, or true skin, is a dense, tough, elastic, and slightly contractile membrane, possessing extreme sensibility, and abundantly supplied with vessels. It offers two surfaces for our inspection;—the internal, firmly connected to the subjacent parts, presents the appearance of a net-work of ligamentous fibres, in the interstices of which are lodged nerves, vessels, and globules of fat;—the external surface is marked by ridges, running in a slightly curved direction, and composed of an innumerable number of minute conical eminences or papillæ. So far all anatomists are agreed; but its intimate structure has remained a secret until very lately. Many of the older writers believed that the entire skin was formed of the meninges of the brain, conducted thither by the nerves; and though this absurd opinion soon became exploded, many succeeding anatomists were satisfied with asserting, that its structure was inextricable, while the accounts given by others were of the most contradictory nature.

More recent investigations, and especially the late elaborate researches of Breschet[1] and of Gurlt,[2] have, however, thrown so much new light upon the subject, that it bids fair to be soon satisfactorily made out; and accordingly in the

[1] *Breschet.* Nouvelles Recherches sur la structure de la Peau, 8vo. Paris, 1835; or his two Memoirs in Ann. des Sc. Nat. ii. 1834. (The references in this essay are always to the first mentioned work).

[2] *Gurlt.* Müller's Archiv. 1835.

following notice, I shall in a great measure confine myself to their descriptions. The papillæ, which were first but very imperfectly described by Malpighi, in his treatise, "De externo Tactus organo," published in 1686; of which more accurate, but still incomplete accounts were given by Ruysch, Saeretaire, and Boerhaave; which Gaultier, from their extreme vascularity, denominated "bourgeons sanguins;" which Steller and de Blainville mistook for agglutinated hairs; and which Rapp considered as true secreting organs, analogous to the duodenal flocculi, are thus described by Breschet.[1] They are arranged in continued rows; are most commonly bifid or trifid, and in form resemble blunt cones, the summits of which are imbedded in the superjacent epidermis, while their bases are lost upon the general surface of the true skin. Their direction in the epidermis is oblique, and slightly inclined, and their summit is not pierced by any foramen. So far this description accords with my own observations; but Breschet also speaks of a kind of sheath, or neurilema, which the papillæ derive from the external surface of the true skin. Of the existence of this, as a separate structure, I have never been able to convince myself, and am rather inclined to believe, that the opinion rests upon inaccurate observation, the uninjected plexus of lymphatics and bloodvessels having been mistaken for an independent sheath. And this opinion is supported by the fact, that Breschet appears in his description to have attributed too little vascularity to the papillæ, probably from not having been altogether successful in his injections.

The lymphatic plexus, of which but little notice is taken in the work from whence this account has been derived, is fully described in a subsequent publication by the same author.[2] He there confirms the preceding observations of Panizza, Mascagni, Gordon, &c., in speaking of it as a deli-

[1] Loc. cit. p. 10, et seq.

[2] *Breschet.* Le système Lymphatique, considéré sous les rapports Anatomique, Physiologique, et Pathologique, 8vo. Paris, 1836, p. 29, &c.

eate network, interwoven with the capillary bloodvessels, but for the most part lying rather more superficial. After the minutest examination, he was unable to discover any traces of open orifices, nor could he see any ramuscule, with free extremities, proceeding from this interlacement; but he found prolongations or loops projecting beyond the level of the other parts, and imbedded in the substance of the epidermis. This latter observation, as will be seen immediately, throws great doubt upon certain points in his account of the epidermis. But to return to our examination of the papillæ.

The extreme sensibility of the skin soon induced anatomists to conclude *a priori*, that they must be abundantly supplied with nerves. Ruysch was evidently acquainted with the fact, for he says, in describing a plate in the first number of the Thesaurus Anat.:—"Tab. iv. fig. 9. Theas. Anat. i., repræsentat portionem papillæ uberis Balænæ, cujus papillæ cutaneæ à me dissolutæ sunt in aqua simplici, id est, singulæ papillæ ex multis fibris nervosis constitutæ quidem sunt. In homine autem hanc dissolutionem papillarum cutanearum nunquam peragere potui, utpote fibris sibi invicem firmius cohærentibus."

Sacretaire[1] appears to have seen the union of the nervous filaments into loops, for in describing the papillary bodies he has the following words:—"In his enim partibus locantur corpuscula quædam, composita ex quatuor, quinque vel plurimis quasi filamentis è cute surgentibus, suis extremis inter se connexis, certoque ordine disponuntur."

Boerhaave[2] dissected the subcutaneous nerves as far as the skin. He objects to the once commonly received notion, that the three parts of the skin were formed by the neurilemas of the nerves, which had followed them from the meninges of the brain, and remarks, "Hi (nervi subcut.) quoque propriores cuti, eo fiunt teniores, donec ex hisce minimi, innu-

[1] *Sacretaire.* Diss. Med. Inaug. de Com. corp. hum. Integ. 1727.

[2] *Boerhaave.* Persp. Dict. Hippoc. &c., 12mo. Lugd. Bat. 1738.

meri adsurgunt ramuli, qui cutim intrant, ad papillas tendunt, et in iis ordinantur. Imo vero has huc usque servare quibus investiuntur tunicas affirmo. Eo usque prosecutus sum, microscopio et acu tenuissima, cujus attactum in hoc loco ferunt."

And our author has confirmed these previous observations, for he describes numerous filaments, bearing all the characters of nervous matter, as entering the base of each papillary eminence, proceeding upwards in the form of undulated striæ, and uniting at the summit by loops, but not sending off any lateral anastomosing branches. Whether in this situation they still possess, or are destitute of their neurilema, he professes himself incapable of affirming with confidence, but inclines to the latter opinion.

Such being the structure of the papillæ, as far as can be yet ascertained, I proceed now to describe the organs, by means of which the important exeretion of sweat is eliminated from the body, and which, until the publication of Breschet's work, were utterly unknown. His observations on this subject have been since amply confirmed by Gurlt, and for their correctness I can also vouch from frequent examination.

The exhalant, then, or diapnogenous apparatus, as he would call it, consists of a secreting gland, situated in the dermis, in form resembling a sac, somewhat distended, and surrounded with capillary bloodvessels. From its summit a spiral tube proceeds, traversing the fissure between the papillæ, and passing through the epidermis, in a very oblique direction, at the external surface of which it opens, this point being indicated by a slight depression or pore. It will be seen at once, that the nature of its course prevents the possibility of any foramen appearing, when the cuticle is raised from the skin, the lacerated ducts immediately retracting, and the apertures being closed, by the contact of the upper and lower parietes of the tube. A very beautiful and conclusive proof of the correctness of this account may be readily obtained, by observing with a glass the transudation of the sweat, as it

occurs at the palms of the hands, or the tips of the fingers, when the first drop will be seen to be preceded by an elevation of the cutiele, after the manner of a valve. The filaments, therefore, which are perceived stretching between the cutiele and eutis, when these two membranes are separated, and which have been denominated by various authors, absorbents,[1] exhalants,[1] nerves,[2] or films of mueus,[3] are now shewn beyond doubt to be the excretory duets above described; and it is equally eertain that by their means alone is exhalation effected. These ducts, according to several measurements which I made, have a diameter of $\frac{1}{150}$ inch, the canal oeeupying about one-third of their breadth; they present a peeuliar and very distinet spotted appearanee, and are eapable of being stretched to a eonsiderable extent before yielding. They may be most readily examined by making a thin vertieal section of a piece of the skin of the heel, which has been steeped for some time in a saturated solution of bicarbonate of potass, and then gently separating the cutiele from the cutis. Under these eireumstances they are even visible to the naked eye, stretching between the two membranes, or floating in the water with whieh the object-glass of the mieroseope is moistened.

So far, then, the strueture of the dermis may be considered as satisfactorily aseertained, but there are still some points concerning which eonsiderable doubt exists. Thus Breschet has deseribed a set of glands destined to seerete the horny matter of the euticle, and another vaseular and spongy apparatus for the produetion of scales, on which its colour depends;[4] but neither of these observations have been eonfirmed by Gurlt, and my own investigations into the subject have

[1] *Bichat.* Anatomie Generale, iv. 759; and, *Hunter.* Med. Obs. and Inquiries, ii. 52, for 1762.

[2] *Monro.* On Structure and Function of Nervous System, plate xli. fig. 5, folio. Edin. 1783.

[3] *Gordon.* Loc. cit. 237.

[4] *Breschet.* l. c. 72, et seq.

been equally unsuccessful, Indeed I am much inclined to believe that Breschet has mistaken for a new structure some of the ordinary sudoriferous glands, whose ducts had by some means been broken off at the surface of the dermis, and that the vascular spongy apparatus is nothing more than the network of vessels which composes the greater part of the most external portions of the cutis.

The sebaceous glands, the hairs, nails, and other appendages of the skin, being quite unimportant in our present inquiry, may be passed over in silence; and we proceed, therefore, at once, to the consideration of the cuticle, a correct apprehension of its structure being of the greatest possible moment.

CHAPTER III.

OF THE CUTICLE.

The greatest difference of opinion always has, and continues still to exist, concerning the structure of this membrane, at first sight so simple, and apparently homogeneous. Thus the older anatomists, and especially Vesalius, have described it as an efflorescence of the true skin. Morgagni considered it as an external lamella of the body, compressed by the air into a membranous form. Boerhaave imagined it to be formed by a reunion of exhalants. According to Ruysch it is an efflorescence of the nervous papillæ.[1] Haller and Garengeot hold it to be a thickened portion of the corpus mucosum.[2] Leeuwenhoek, Baker,[3] and Bichat considered it scaly. Beclard, a plane continuous membrane. De Blainville, a horny excretion. Mojon maintains that it is both organic, and possessed of vital properties, being formed by an interlacement of cellular filaments, exhalants, and absorbents, in a matrix of albuminous matter.[4]

Delle Chiaje adopts the extraordinary opinion, that it is composed of the globules of the blood, deprived of their fibrin, and dried.[5] While Magendie,[6] and some others, assert that it is nothing more than a species of protecting varnish. M. Collard de Martigny inclines to the opinion, that the epider-

[1] *Breschet.* l. c. 95, et seq.
[2] *Haller.* Elementa Physiologiæ, v. 20.
[3] *Baker.* On Microscopes, &c. vol. i. 172, 8vo. London, 1785.
[4] *Breschet.* l. c. 99.
[5] *Breschet.* l. c. 99.
[6] *Magendie.* Leçons, &c., p. 35.

mis, *truly* organised, is formed by the membranous expansion of the external orifices of absorbents and exhalants, without the participation of nerves; an idea founded, 1. Upon the vital action of which it is the seat. 2. Upon its little sensibility. 3. Upon its scaly and lamellar appearance under the microscope. 4. Upon its adherence, loose, contractile, and filamentous, with the true skin. And, 5. Upon its mode of formation and regeneration.[1]

According to Fontana, a fine lamina of the cuticle examined by the microscope, presented the appearance of a web of winding cylinders, which approached each other, and retreated, with great order and regularity, and were interspersed at various places with very small globules. "I employed," says he, "glasses which magnified 700 times, but could perceive nothing more. Neither hole nor porosity could be discovered. It is very probable that the lymphatic vessels, which Father della Torre says he observed in the epidermis, in the form of network, are no other than my winding cylinders."[2]

Dr. Gordon, again, denies that any appearance of scales, laminæ, or fibres, can be discovered even under the closest inspection.[3]

The late Mr. Chevalier of London, in a course of lectures delivered before the Royal College of Surgeons, has described the structure of the cuticle with great minuteness.[4] His observations were conducted with a microscope magnifying 140 times, and gave the following results. No appearance of pores could be detected, excepting where it was evident that hairs had passed through, but in their place he found "an infinite number of minute velamina, regularly arranged,

[1] *Collard de Martigny.* Arch. Gen. de Méd. xi. 73.

[2] *Fontana.* On the Venom of the Viper, &c. vol. ii. Translated by Skinner, 8vo. Lond. 1787.

[3] *Gordon.* System of Anatomy, i. 237.

[4] *Chevalier.* Lectures on the General Structure of the Human Body, 8vo. Lond. 1823.

of exquisite tenuity, presenting a follicular appearance, and separated from each other by bands of a thicker substance, crossing and intersecting them."[1] Of this appearance he has given drawings, which, as far as my own observations go, present a tolerably exact representation of the parts; but as will be seen immediately, the explanation he has offered requires considerable modification. The terminal vessels of the skin he believes to be lodged in these velamina; one set transmitting their secretions outwards, while another, running in an opposite direction, carry on the process of absorption.[2] The so-called rete mucosum, of which so many contradictory accounts have been given, and the very existence of which is more than doubtful, is denominated by Mr. Chevalier the internal epidermis, and described to be of the same structure as the external, only more loose in texture.[3] He even hazards a suggestion, that it may "constitute one wide and diffused perspiratory gland,"[4] an idea totally unsupported by fact or observation. Between this and the external layer he describes a set of minute inter-epidermal glands, to which he attributes the same function as the sebaceous follicles exercise.[5]

Few anatomists have followed Mr. Chevalier in this description; but Dr. Wallace of Dublin, in some lectures on the structure of the skin of the negro, as unfolded by the application of blisters, lately published in the Lancet, (Nov. 25, Dec. 2 and 9, 1837), has contended strenuously for the existence of these epidermoid glands. He affirms that the cutis anserina is produced by their unusual prominence; that lichen, strophulus, and some other cutaneous diseases, are the results of their inflammation, and that their normal function is the excretion of sweat; this opinion being founded upon the fact of that fluid being observed to exude from the surface of the cuticle, in points precisely corresponding to their

[1] *Chevalier*. l. c. 133.
[2] Pp. 134, 5.
[3] P. 265,—description of fig. 5, plate ii.
[4] P. 164.
[5] P. 184, and plates iii. and v.

supposed location. The situation of these bodies, however, —their regular arrangement along the ridges of the cuticle, —the circumstance of their ducts being said to open, precisely where the sudoriferous canals, already described, make their exit,—and the very important fact, that they are only visible in a horizontal section, no such appearance whatever being presented when the epidermis is cut vertically,—all these considerations incline me to believe, that both Mr. Chevalier and Dr. Wallace have been deceived, and that their supposed glands are nothing more than the shrunk and contracted ducts of the true secreting organs of the perspiration.

Dr. Wallace expresses his astonishment that Breschet has not noticed these bodies. The reason probably is to be found in the fact stated above, viz., that the skin, when examined in the way Breschet recommends, does not exhibit any such structure; and if he did see them after the sudoriferous ducts had been clearly made out, he could be at no loss to attribute the appearance to its true source.

The description of the epidermis given by this last mentioned author, is very minute, and of considerable importance, though probably erroneous in many respects. According to his researches, it would appear to possess a somewhat complex structure. The internal surface, the mucous layer of Malpighi, softer than the external, is moulded round and over all the inequalities of the true skin, for which it constitutes a perfect sheath. Examined by the microscope, it seems composed of a multitude of irregular scales, placed upon a thin areolar membrane. This structure, though sufficiently obvious in man, is yet much more fully developed in the whale, the epidermis in this animal being made up of perpendicular fibres, the extremities of which are bent into a horizontal direction, and which are themselves composed of a series of scales, bearing a determinate shape. But the part of its structure with which we are most concerned, is the existence of vessels. We have already seen that this membrane cannot be strictly termed extravascular, inasmuch as it is traversed

by numerous sudorific canals; but if the observations of Breschet may be believed, there exists another set, which he has perhaps somewhat hastily denominated absorbents. These vessels are best seen, by tearing to pieces a small portion of the most external layer of the epidermis, and then viewing it with the glass, when they will be found to present the appearance of isolated radicles, which, after frequent anastomoses, penetrate the dermis by the infundibula of the papillæ, accompanying the sudoriferous canals. All these vascular trunks communicate in the substance of the dermis, under the papillæ, with canals forming a common plexus, and lying at right angles with the furrows. They are very minute, white, and silvery, sometimes exhibiting an appearance of diaphragms, and very friable.

The arguments which he adduces to prove their analogy, both in structure and function, with the absorbents, are very lengthened and ingenious; but they may be passed over in silence, since his more recent observations have shewn that no such terminations of the lymphatic system exist, and since, if the following experiment be correct, they must be considered as forming a part of the sanguiferous system,—"If," says he, "a fine injection be thrown into the principal artery of a limb, this injection stops at the cutis, as has always happened in our injections. If the skin be then cut (en dédolant), and pressure be made with the scalpel, from within to without the injected part, the inhalant vessels of the epidermis become coloured, and are seen ramifying beneath the most superficial layer of the cuticle." This point, however, is of minor importance, the simple fact of any vessels existing in this supposed inorganic varnish would of itself be sufficient for our purpose, whatever their nature might be. I have accordingly taken considerable pains, to endeavour, by personal observation, to ascertain the truth or incorrectness of these views. But easy as the investigation may at first sight appear, it is

[1] *Breschet.* loc. cit. p. 40.

in reality one of no small difficulty, the peculiarly elastic and resisting structure of the part rendering a sufficient division of it extremely troublesome; whilst its uniform semi-transparent appearance under the glass, in a great measure opposes any distinction of its constituents, and immeasurably increases the danger of fallacy. With all these obstacles opposing me, I must confess that my investigations have not led to any such satisfactory results as I had anticipated.

A minute lamina of the cuticle, raised from the palm of the hand by the point of a lancet, and viewed under a compound microscope, with $\frac{1}{6}$th inch focus, did certainly exhibit an appearance closely resembling a capillary network of vessels, so closely indeed, that one can scarcely feel astonished at the conclusion to which Mascagni arrived, viz., that the epidermis is neither more nor less than a congeries of lymphatics. More extended and careful observation has, however, convinced me that this appearance is entirely deceptive, the seemingly minute vascular plexus being evidently produced solely by partial thickenings of the membrane itself; for a thin layer of the compact texture of bone, a minute piece of nail, a slice of a corn from the toe, fine tissue paper, a thin layer of wax, of semifluid gum, and of butter, when examined in the same way, all exhibited appearances precisely analogous,—all bore resemblance to a minute interlacement of fibres, which might be readily mistaken for a capillary plexus of vessels. In my attempts to isolate the vessels, after the manner described by Breschet, I have been equally unfortunate. By tearing to pieces a minute portion of cuticle, under the microscope, I have indeed been frequently able to detect fibres among the scales into which it separates, and at one time I flattered myself that these were the vessels I had so long been seeking; but continued observation again shewed my mistake, and convinced me that they were nothing more than minute shreds of the original membrane. At the same time I do not feel myself entitled to deny altogether the existence of any such vessels, because I have not had an opportu-

nity of repeating Breschet's experiment, related above, which, if correct, is at once conclusive of the point at issue; and because, in separating the cuticle from the cutis, after it had been macerated for some time in the potass solution, I have frequently seen what is represented in fig. 1, viz., fibres bearing much the appearance of vessels, totally distinct, both in size and structure, from the sudoriferous canals, and attached to the epidermis, precisely at those points where Breschet describes his vessels to make their exit. Perhaps, indeed, they may be nothing more than portions of those lymphatic loops which he describes as being imbedded in the cuticle, but upon this I am unable to give any decided opinion.

Dismissing this subject, however, as still undetermined and obscure, I proceed now to give an account of some recent and most interesting additions which have been made to our knowledge of the minute anatomy of this membrane. It has been already stated that Leuwenhoek considered the cuticle to possess a scaly structure. He indeed affirmed that it consisted of these scales alone, which were neatly arranged by one another, and 200 or 270 of which might be covered by a grain of sand.[1] Baker entertained the same opinion. He describes them as so minute, that 200 may be covered by a grain of sand, as being pentagonal, and "placed as on fishes, three deep, that is, each scale so far covered by two others, that only a third part thereof appears."[2] Raspail describes the same structure, believing that the epidermis is not a concreted secretion, but only the most external layer of the cells of the dermis, hardened by the air; and when it exfoliates, it is replaced by a new layer of scales.[3] Gurlt has given a very analogous statement; and Raschkow, following Purkinje, has pointed out the whole matter in these words, when treating of the epithelium of the gums; he says, "it resembles the

[1] *Leuwenhoek.* Arcan. Nat. p. 408; 1719.

[2] *Baker.* loc. cit. i. 172.

[3] *Raspail.* Repert. Gen. d'Anat. iv. 156; or Nouveau Système de Chime Organique, 8vo. Paris, 1833, p. 215.

external cuticle, and like it, but in a more perfect manner, is composed of polyedral squamules, mutually placed upon each other, most of which shew a circular spot in the centre."

But the most full and accurate account which has yet appeared, is one for which we are indebted to Dr. Henle, the friend and pupil of Müller, and of this I shall freely avail myself.[1] The scales in the external parts of the cuticle have a most irregular form, being subrotund, angular, polyedral, and frequently with imperfect or lacerated margins, but all with flat surfaces, frequently bent, and so thin that the microscope discovers innumerable quantities of them, in the smallest particle which the naked eye can see. In these it is rarely possible to discover any traces of a central spot. But from considering the manner in which the cuticle grows, from examining this membrane as it exists in the embryo, and attending to its structure in the adult, where in close apposition to the skin, or where naturally thin and delicate, as on the glans penis and prepuce, he has been led to believe that such are not the regular forms. For in all these situations he found the scales thicker, and bearing the appearance of cells, with straight margins, generally of a pentagonal form, and all provided with a central nucleus.

This nucleus, which is in the centre of each cell, has a smooth, or slightly granulated surface, is subrotund or oval, sometimes appears to contain in it lesser granules, and is thicker than the cell, so that it projects on each side.

The so-called rete Malpighii consists of a similar structure, with this exception, that the nuclei of the same size as in other parts, occupy almost the whole of the cells. Dr. Sharpey informs me that he has been able to convince himself of the accuracy of these statements, and the results of my own investigations into this very interesting subject have been such as to confirm the above observations in the most satis-

[1] *Henle.* Symbolæ ad Anatomiam Villorum Intestinalium, imprimis eorum Epithelii et Vasorum Lacteorum. Comment. Acad. Berolini, 1837.

factory manner. On the internal surface of the skin of the penis, in which situation they are most regular, and exhibit most distinctly the central nucleus, these cells have, according to my measurements, a diameter varying from $\frac{1}{1600}$ to $\frac{1}{2400}$ inch, but more commonly the latter; their form, (as represented in figs. 2 and 3), is subrotund or pentagonal; and that they possess more the nature of *cells* than of *scales*, is shewn by the fact, that they may be seen rolling along the field of the microscope, either without undergoing any change of shape, or exhibiting the lateral projection of the central nucleus. In other parts which I have examined, but more especially on the external surface of the thick cuticular covering of the heel, they are much larger, for the most part about $\frac{1}{800}$ inch in diameter, very irregular in form, and not exhibiting the central nucleus with any distinctness; some however, which had a diameter of $\frac{1}{1600}$ inch, shewed this remarkable structure more clearly. That these, again, are truly *scales*, may be easily determined, by watching them closely as they pass over the object-glass; when it will be seen that many of them, in turning on their axis, become almost converted into cylinders, by the inflection of their edges, and then in the progress of their course, again unfold themselves, and resume their original flattened appearance. In the epithelium of mucous membranes, which, as will be seen immediately, possesses a structure closely resembling that of the epidermis, the cells are united by means of a delicate network of some substance, the precise nature of which is as yet unknown, and we may therefore reasonably conclude that the same means of connection also exist in the parts we are now considering.

It has long been a favourite opinion with anatomists, that the cuticle, and other analogous tissues, are totally destitute both of organisation and vitality; and while these parts were believed to be nothing more than a desiccated secretion, such a doctrine was certainly in some measure probable; but now that we have been able to detect in the cuticle a definite

structure, and have seen that it forms no exception to the rest of the animal body, since it is composed of certain regularly formed bodies, united by a cellular tissue, we must surely feel ourselves compelled to pause before subscribing to this notion. One chief point of difference between organised and inorganic bodies, consists in the fact, that the latter may be divided "ad infinitum," and still present the same structure, while the peculiar characteristics of the former are at once destroyed by such a process; and so it is with the cuticle,—divided to a certain extent, each portion would represent the whole, but carry the separation further, and the entire structure is annihilated. The cells of the cuticle, when first formed, consist of little more than the central nuclei, but in course of time, the outer envelope becomes distinct, and acquires a *great* increase of size, until their original form disappears, and they become converted into scales,—in magnitude so much exceeding the original cells, that it is quite impossible to attribute the change to physical processes alone. In this respect the cuticle bears far more resemblance to bodies of the former than of the latter class. It agrees also with other animal tissues, in presenting different appearances in different parts of the body, these changes being evidently adapted to the specific purposes which it is destined to fulfil, being totally inexplicable by a reference to the subjacent parts, and evidently not, as some have imagined, the effects of extraneous circumstances, such as pressure, for they exist even in the fœtus in utero.[1] Organised bodies, during life, are liable to constant change, either produced by the regular progress of time, or by disease; and the same is true of the cuticle, as witness the difference between that membrane in early youth, and in old age, and the remarkable morbid alterations which it often undergoes. It is indeed well remarked by Dr. Wallace, that in the consideration of this very interesting and still little

[1] Vide *Wallace*. Lancet, Dec. 9, 1837, where diagrams are given, illustrative of this point.

understood class of diseases, the attention of pathologists has been too much directed to the true skin, as the only seat of the affection, for many even of the most formidable in appearance, as, for instance, that which he has described under the name of morulus, from the resemblance which the tubercles bear to mulberries, are evidently dependent upon a morbid change of this texture alone, the true skin presenting in every respect its natural appearance, and when the diseased parts have been removed, becoming again covered with healthy and normally formed cuticle. If, from the various and often sudden changes which it undergoes, we allow "that vital processes of nutrition and absorption"[1] are carried on in the substance of hair itself,[2] a production so analogous, both in structure and composition, to the cuticle, why should we deny the same degree of vitality to this membrane, which at least exhibits an equal amount of change? The absence of nerves, and the consequent insensibility, cannot be taken as any argument to the contrary, since upon this ground we must extend the same objection both to cellular tissue and tendons, the vitality of which few will be inclined to question.

But, lastly, and which is of the greatest importance, organised tissues, so long as they are attached to the living bodies of which they form a part, possess the remarkable power of withstanding, to a certain extent, the influence of chemical agents; and that the cuticle forms no exception to the general rule, is sufficiently proved by the following very interesting experiments, for the performance of which we are indebted to Dr. Wallace. He immersed a plate of the epidermoid tissue of a negro in a saturated solution of chloride of

[1] *Alison.* Outlines of Physiology, p. 95.

[2] NOTE.—The most remarkable case of this kind which I have ever seen recorded, is that of a young lady whose hair suddenly broke off, at a quarter of an inch from the head, and came away in large locks. Since the first time, this remarkable process has been repeated every three or four weeks, the hair growing rapidly in the intermediate periods.—Vide *Mayo's* Physiology, 4th edit. Lond. 1837, p. 468.

lime, and its deep black colour was almost immediately discharged. He applied the same solution to the surface of the cuticle attached to the skin, but after the lapse of one hour, the blackness was deepened instead of diminished. A continuation of the same application produced much local excitement, caused the epidermis, though still retaining its black colour, to become wrinkled, and its attachments loosened; and when this same cuticle was finally separated, and immersed in the solution, it at once became permanently whitened. After removing the most external layers of the cuticle, the surface of the skin (still covered by epidermis) was treated in the same way, and with precisely similar results. The negro was then made to wash his hands in the solution, but no effect was produced, excepting in those parts where flakes were separating from the thickened membrane.

A careful and unprejudiced contemplation of all these circumstances can, it appears to me, lead only to the inevitable conclusion, that this membrane is both organised, and possessed of vitality; and however heterodox the opinion may seem to many, I must confess, that even independently of the proofs just related, it has always appeared to me far more consonant with all that we know of the animal economy, than to imagine that a layer of inorganic and dead matter could remain attached to the whole surface of the skin, without producing that tendency to a final and complete separation of the one from the other, which we invariably observe in all other parts, between living and dead matter.

But dismissing this subject, let us for a moment consider the opinions of those who hold that the cuticle is also impermeable to fluids, and see how far observation and experiment will warrant such a conclusion. Every one must have observed, that after immersing the feet for some time in hot water, the thick cuticle of the sole becomes whitened and opaque, and that pressure will then cause a quantity of fluid to exude; and every body is acquainted with the exquisite hygrometric properties of that analogous structure, hair; nei-

ther of which could take place, were this substance impermeable. But direct experiments are not wanting, to prove the utter incorrectness of this doctrine. Lebküchner has shewn that substances applied to the skin of animals after death, penetrate to the interior of the body with various degrees of rapidity.[1] Thus, ol. tereb. and camphor, placed on the skin of a rabbit, twelve hours after death, communicated, in the space of ten hours, their peculiar odours to a paper placed on the internal surface of that membrane. A solution of prussiate of potass penetrated in five hours; sulphuric acid in six hours; and acetic acid in twenty-four hours. Ink, and a solution of muriate of soda, had not passed in twenty-four hours; and a solution of ammoniaret of copper required two days for its transit. Magendie immersed the paw of a rabbit in ink, and the cellular membrane became coloured.[2] He formed a bag from a piece of human skin, the epidermis being internal, and then filled it with water,—transudation took place rapidly; but when the experiment was reversed, the epidermis became raised, and bullæ were formed,[3] thus clearly shewing that the passage of fluids takes place much more rapidly in one direction than in the other. Surely these facts are enough to prove how insufficient are the grounds upon which this opinion has been founded; and to demonstrate the absurdity of trusting to vague conjectures, in place of at once appealing to experiment.

It only remains, then, that before quitting this anatomical part of my subject, I should very briefly notice the analogy between the skin and mucous membranes. This has been differently stated by various authors. Cuvier distinctly affirms that the membrane lining the alimentary canal is nothing more than a continuation of the skin, consisting of the same

[1] *Lebküchner*. Diss. Inaug. utrum per viventium adhuc animalium membranas, materiæ ponderabiles permeare queant. Reported in Arch. Gen. de Med. vii. 424.

[2] *Magendie*. Leçons, &c., p. 28.

[3] L. c. pp. 90, 91.

parts, and performing the same functions.[1] Lieberkühn described a membrane like the cuticle, as covering the surface of mucous membranes. (Diss. de Fabrica et Actione Villorum. Amstelod. 1745.) Mascagni has noticed the same structure, and imagines it to consist of lymphatics alone. Rudolphi also believes that the "epithelium," (which all acknowledge to exist to a certain but variable distance along the canal), is continued throughout the whole extent of the passages; and in this he is supported by Dr. Elliotson, and some later writers, but is altogether opposed by Bichat.[2] My friend, Dr. Boyd, in an admirable essay on the mucous membrane of the stomach, has described this structure with considerable accuracy. He has shewn that it is moulded round, and forms a covering for the papillæ of the subjacent membrane, in a manner precisely similar to that which is exhibited by the cuticle, as we have before explained; and in the horse it even presents an appearance not unlike that which has obtained the name of rete mucosum, an intermediate perforated layer being visible beneath the most exterior. When carefully examined by the microscope, it is seen to possess a structure in some parts precisely similar to that of the epidermis, in others analogous, though somewhat modified. Thus Leuwenhoek discovered scales in the mouth, of the same nature, but larger and softer than those of the skin. Purkinje and Valentine have described and figured similar bodies, and the observations of Henle have settled the question most satisfactorily. In the conjunctiva, and the mucous membrane lining the mouth and œsophagus, the scales and cells present the exact resemblance of those which we have already described as existing in the skin. This will be at once seen upon a reference to figs. 4, 5, and 6. At the cardiac orifice of the stomach these cells disappear, and are replaced by cylindrical bodies. They again re-appear in the body of the

[1] *Cuvier.* Animal Kingdom, by M'Murtrie, 8vo. Lond. 1834, p. 18.

[2] Vide *Boyd.* On the Structure of the Mucous Membrane of the Stomach.—Edin. Med. and Surg. Jour. Nov. 1836.

stomach, and are finally lost at the pylorus; the whole remaining extent of the intestinal mucous membrane being covered by cylindrical or cuneiform bodies, united by their sides, and fixed to the subjacent tissues by their apices. These cylinders are connected by a network of some unknown substance, and have often, though not always, the central nucleus; their exact figure and arrangement will be seen by a reference to the plate. In the air passages the vibratory cilia are placed upon analogous cylinders, (fig. 12), an additional proof, if such were needed, that they are truly organised, for although it has appeared probable to many, that a layer of inorganic matter should cover the surface of a living tissue, few, I apprehend, would be inclined to hold the same opinion with regard to one which was interposed between two living, organised structures; and such are both the mucous membrane and the vibratory cilia.[1]

Cells of the same nature as those in the mouth have also been detected by Donné and Turpin in the genito-urinary passages;[2] and my own observations, as far as they go, entirely confirm those of Henle. In the mouth, from the surface of which they are easily separated, those which I measured had about the same diameter as those of the skin of the penis. In the intestines, the cylinders which cover the surface of the jejunum, have their longest diameter varying from $\frac{1}{750}$ to $\frac{1}{800}$ inch; in the duodenum they are smaller.

Were any other evidence necessary to complete the analogy, it would be found in the well known fact, that when a portion of the upper lip has been taken to form the columna nasi, the mucous membrane thus exposed to the air has gradually acquired all the properties of the ordinary integuments.[3]

Such, then, being the resemblance in structure, we shall

[1] Vide *Sharpey*. Cyclop. of Anat. and Phys.—art. "Cilia."

[2] Ann. des Sciences Nat. Mai 1837, p. 207.

[3] *Liston*. Elements of Surgery, part 2.

not be surprised to find that these tissues exercise corresponding functions; and having thus seen how unfounded must be all objections based upon the alleged want of vitality, inorganic nature, and impermeability of the cuticle, we are prepared to enter upon the consideration of the absorbing power of the skin, with minds unshackled by these prejudices. We are willing to trust the decision of the question to the only sure and unerring test of experiment, always remembering the important maxim, "on s'égare presque toujours, quand on veut diviner la nature, au lieu de l'observer."

DIVISION II.

CHAPTER I.

ABSORPTION IN THE BATH.

In pursuanee of the plan already laid down, I now enter upon the proper business of this essay, commeneing with the absorption of water in its liquid form. Under this head, I shall eonsider the effects of baths, both loeal and general, pure, and eontaining nutrient matters; earefully reviewing all that has been already written upon the subjeet, as far at least as I have been able to learn, and adding the results of my own experience. The paramount importanee of this part of our undertaking will be at once evident, when we remember, that the arguments considered by the opponents of eutaneous absorption as most eonvineing, have been derived from experiments performed in the warm bath. Whether the conclusions drawn from them are eorreet or unfounded, and whether the evidence derived from baths is of the most convineing nature or not, we shall not now stop to inquire,—the answer will be found in the sequel. But the great stress whieh many physiologists have laid upon them, renders it imperative that the question be fully diseussed. I prefer, therefore, rather to incur the eharge of prolixity, than to run the hazard of leaving my task imperfeet, by aiming at too great coneiseness.

To begin, then, with the consideration of local baths. The earliest account which I have been able to find, of absorption taking place from the partial application of water to the surface, is a most remarkable, indeed scarcely to be credited, case, detailed by Simson, in his treatise "De re Medica."[1] "A young man," says he, "was seized with a fever, upon which a diarrhœa supervened, with great stupor, and he refused to drink anything, though he was parched up with excessive thirst. The better to supply him with moisture, I directed his feet to be immersed in cold water; immediately I observed a wonderful decrease of water in the vessel, and an impetuous stream of a fluid scarcely coloured was discharged by stool like a cataract." Dr. Percival[2] affirms that one of his hands, after being well chafed, imbibed nearly ℥ifs of water in the space of a quarter of an hour. Falconner made a number of experiments upon absorption in warm water.[3] He found that some hours before taking food, water at 112° F., in which his hand was immersed, and which cooled down to 91° F. during the experiment, lost 98 gr. in a quarter of an hour, allowance being made for evaporation. In a second trial, the loss during the same period was only 64 gr. Similar experiments performed after eating, gave a loss of only 38 gr.; and when the air was very warm, and consequently exhalation from the surface of the skin extremely copious and rapid, the water had absolutely *gained* a few grains. Taking the results of these experiments as a standard, he computes that the absorption from the whole surface of the body would cæteris paribus range from four to twelve oz. in the quarter of an hour; a calculation which succeeding observation has, however, proved to be perfectly erroneous. Analogous expe-

[1] *Simson.* De re Medica, 8vo. Edin. 1726, p. 183.

[2] *Percival.* Trans. of Col. of Phys. Lond. ii. 102; or vide *Darwin.* On the Retrograde Motion of the Lymphatics. Lichfield, 1780, p. 50.

[3] Vide *Marcard.* De la Nature, et de l'Usage des Bains, 8vo. Paris, 1801, pp. 168, 169.

riments had been previously instituted by Dr. Alexander,[1] but they were by no means so conclusive, since he omitted to take into account the loss produced by evaporation.

Maseagni observed that from keeping his feet during some hours in water, there resulted tumefaction of the inguinal glands, the exudation of a fluid from the glans penis, and immediately afterwards a violent coryza; to account for which phenomena he offers the following most ingenious, though somewhat fanciful explanation:[2] "Cum pedum lymphatiea insolita humoris copia obrueret, eoque glandulæ turgescerent, hinc lymphatica penis, quæ in glandulas inguinales communem habent cum primis sui fluidi exitum, eodem difficilius se exonerabant. Vasa sanguinea eamdem humoris quantitatem, deponere pergebant, hunc vero avehere totum non poterant vasa lymphatica, utpote quæ proprium fluidum ferebant motû retardato; itaque ex penis glande humor reliquus exstillabat. Similiter, cum ex abundante lymphaticorum ad pedes pertinentium absorptione, magna fluidi vis ductum thoracicum distenderet, minime fieri poterat, quin ex hoc, pituitariæ lymphaticis sinistri lateris, impedimentum occurreret. Qua propter in narium cavitates, â vasis sanguinis effusus, neque â lymphatieis copiosè satis absorptus, in tenuis coryzæ formam effluebat." The same effects he affirms to have been produced by moist air blowing upon his feet or arms; and he concludes in these words:—"Itaque satis superque demonstratum existimo, substantias â superficiê externâ, lymphaticorum actione et ministerio, in corpus introduci."

Notwithstanding the importance which must undoubtedly be attached to the direct evidence of an anatomist so celebrated, an observer so accurate as Mascagni, yet all candid inquirers must be compelled to acknowledge that his testimony on this head is far from satisfactory. We cannot, indeed, imagine that the occurrences are incorrectly reported, but the accuracy

[1] *Alexander.* Experimental Essays. Lond. 1768, p. 26.

[2] *Mascagni.* Vasorum Lymphaticorum Corporis Humani Historia et Ichonographia, folio. Senis, 1787, p. 23.

of the explanation is, to say the least, extremely hypothetical; the coryza certainly may have had a very different origin. There is also a fatal objection which may be urged against the other experiments we have related, viz., that in none of them was the quantity of fluid noticed, which must have adhered to the surface of the parts immersed. Their results, therefore cannot be deemed strictly accurate, and indeed but little reliance can be placed on any observations so loosely conducted. But all these deficiencies are amply supplied in the elaborate and careful researches of Collard.[1] He confirms the observations of Mascagni, as to the tumefaction of the lymphatic glands, from immersion of the limbs in water, and relates a number of experiments, of which the following is an abstract. By means of a very sensible balance, capable of turning with a quarter of a grain, he carefully weighed a handkerchief. Removing this, he placed in each scale a china vessel of the same diameter, established an equilibrium between them, and then poured into both an equal quantity of tepid water. One of the vessels was then removed from the scale, and replaced by weights equal to the other, and thus the quantity of water employed was determined. These circumstances were all carefully noted. The weight of the handkerchief and of the water being thus known, and a perfect equilibrium established between the two vessels, he plunged his arms up to the elbows in one, leaving the other untouched, remained thus for half an hour, then dried his arms with the handkerchief, and weighed it immediately. Having now replaced the vessels under precisely the same circumstances as before, he found the one in which his arms had been immersed considerably lighter, and accordingly added weight to restore the equilibrium. The experiment was performed six and-a-half hours after taking food; the air was cold and dry, the day serene, and the temperature of the water 74° F. It gave the following results:—

[1] *Collard de Martigny.* Arch. Gen. de Med. x. 304; and xi 73.

Weight of the handkerchief,.....	before,	℥ii + gr. xiv.
	after,	℥ii + gr. xl.
Increase,.....		gr. xxvi.
Weight of each vessel full of water in equilibrium,—before,.....		℥xii + ʒiv + gr. xxiv.
Weight of lightest,—after,.....		℥xii + ʒii + gr. lxvfs.
Total loss,.....		gr. $104\frac{1}{2}$
Water absorbed by handkerchief,		gr. 26
Absolute loss,.....		gr. $78\frac{1}{2}$

I give the account of this experiment at full length, because all sources of fallacy appear to have been carefully guarded against, and because it is evident that the loss of water could have been caused by no other means than absorption, since evaporation was proceeding equally from both vessels, and indeed from the greater extent of surface exposed, may be reasonably imagined to have gone on more rapidly in the one which was untouched. But his observations did not stop here. Having procured a glass funnel, the diameter of whose base was twenty-five lines, he sealed the end of the tube, and filled it with water. Applying then the palm of his hand to the surface of the water, he inverted it, taking care that none of the fluid escaped, and that no air entered. At the expiration of half an hour the integuments appeared tumefied, and resisted the removal of the funnel, as if a vacuum had been formed. On a repetition of the experiment, somewhat modified, a bubble of air being allowed to remain in the tube, which was also graduated, in three quarters of an hour, no swelling had taken place, but the surface of the water had become sensibly lower. The next affords, if possible, still more conclusive evidence. He took a tube of glass, bent like a syphon, and widened into a funnel at one extremity; some mercury was placed in the bend, and the limb near the funnel was filled with water. The palm of the hand was then applied to it, and retained there for the space

of two hours, at the expiration of which period the column of mercury had risen considerably towards the hand, thus clearly indicating the absorption of some part of the water. Milk, tried in the same way, was absorbed less quickly, soup more rapidly than water. In some subsequent experiments, detailed in the Bibliot. Med. for July 1827,[1] he found that oleaginous matters are not absorbed by the skin, even when exposed to it for the space of twenty-four hours; and this result is quite consonant with many other observations to be afterwards noticed. The obstruction to the passage of blood through the capillaries, and consequent inflammation, which Bichat and Magendie have shewn to result from the direct introduction of oil into the blood, and the fact that when taken into the stomach, it undergoes a species of digestion, and when exposed to serous surfaces becomes saponified, before being absorbed, may perhaps in some measure account for the singular circumstance of its rejection by the skin, since no such change can possibly be effected in that situation.

It may be well to notice in this place the results of Dr. Edwards' observations, since, though of a somewhat different nature, and referring principally to the lower animals, they yet afford the most convincing proof that water, when applied to a part of the surface, finds its way into the interior. A fish which had been wiped and then weighed, was suspended in a limited quantity of water, so that its head and gills were above the surface. It died in nine hours and twenty minutes, and being then weighed again, was found to have undergone no sensible diminution, but to have rather increased in weight.[2] A lizard was exposed to the open air for several days, until it had sensibly lost weight. It was then introduced into a tube, and fastened by a hind and a fore foot, and then placed in water, so that the hind legs and the posterior part of the trunk were alone immersed. It was afterwards weighed at

[1] Vide Review in Edin. Med. and Surg. Jour. xxix.

[2] *Edwards.* On the Influence of Physical Agents on Life. Translated by Dr. Hodgkin, p. 61, 8vo. Lond. 1832.

distant intervals, and found to have successively increased in weight, until it had supplied the loss incurred by perspiration in the air. "This absorption," says Dr. Edwards, "was not mere imbibition limited to the surface; the water penetrated deeper, and was distributed through the system. The body and the limbs had resumed their roundness and plumpness; and life, which would ere long have been extinguished in the air, as had happened to several other individuals which were exposed at the same time, was prolonged by the liquid, which absorption at the external surface had furnished, to repair the loss which had been sustained."[1]

A similar result has been also obtained by Dr. Barton, who found that frogs confined in dry glass vessels became enfeebled, diminished in size, and unable to leap, but that on the introduction of a small quantity of water they soon acquired their wonted vigour, became plump, and as lively as usual in their motions.[2]

Schreger has performed an experiment, in which a bandage having been tied round the hind leg of a puppy, close to the pelvis, the limb was kept during twenty-four hours in tepid milk. At the expiration of this period, the lymphatics were found full of milk,—the veins contained none; and in repeating it upon a young man, no milk could be detected in the blood drawn from a vein.[3]

The experiments which I have myself performed upon this part of the subject, tend decidedly to corroborate the testimony of those above related. They are as follow:—

Experiment I.—Having procured an apparatus, consisting of a large glass jar, to which a syphon-like tube was attached, a quantity of lukewarm water was poured into it, and the free end of the tube immersed in a vessel of coloured water. I then plunged my arm into the jar, and had the surface of the water

[1] L. c. p. 182. [2] Vide *Klapp.* Inaug. Essay, p. 30.
[3] Vide account by *Seiler* and *Ficinus*, in Jour. Compl. xviii. 327.

covered by a layer of oil, to prevent evaporation. At the end of thirty-five minutes, the fluid had risen in the free, and sunk in the attached limb, to the extent of one-eighth of an inch; the levels at the commencement having been accurately marked by sticking plaster. Now, since the lowering of the water in the tube could only take place to the same extent as in the larger vessel, and since the diameter of the tube was only two lines, while that of the vessel was three inches, it is quite clear that a very considerable absorption must have taken place. The temperature of the fluid was rather raised than lowered at the end of the experiment, so that the diminution cannot be attributed to contraction.

Experiment II.—I repeated the same experiment, and in a quarter of an hour the fluid in the tube had descended nearly a quarter of an inch. I did not in this, or any of the following experiments, make any use of the coloured water in the small vessel, since it was troublesome, without rendering the results more conclusive.

Experiment III.—I repeated the same experiment, but from an error in the performance of it, the surface of my arm was covered with a layer of the oil, and consequently in half an hour no absorption had taken place.

Experiment IV —I repeated the same experiment, taking care to avoid the accident which occurred in the last. In a quarter of an hour the level of the water in the tube had sunk three-tenths of an inch.

Experiment V.—Employing the same apparatus, I immersed my arm in milk diluted with a small quantity of warm water, and the surface covered as before with a layer of oil. To ensure still greater accuracy, and obviate any chance of error, which might arise from not keeping my arm invariably immersed to the same extent, the point corresponding with the edge of the jar

was carefully marked at the commencement of the experiment, and these parts were again adjusted at the termination. Proceeding in this way, I found that at the expiration of twenty-five minutes the fluid had sunk half an inch in the tube.

Experiment VI.—I repeated the same experiment with strong and fat beef-tea. At the end of half an hour, not the slightest absorption had taken place,—a result which was evidently dependent upon the greasy nature of the fluid.

Experiment VII.—I repeated the last experiment, with precisely the same result, no absorption having taken place at the end of twenty minutes.

Experiment VIII.—I performed a similar experiment with undiluted warm milk, and in twenty-five minutes the fluid had descended a quarter of an inch in the tube.[1]

The results of these latter experiments do not altogether coincide with the observations of Collard, before noticed; but perhaps the difference may be greater in appearance than in reality, for he does not give us any information regarding the nature of his "bouillon," which may, for aught we know, have

[1] Note.—Since writing the above, at the suggestion of my friend, Dr. Macdonald, I have performed the following experiment. My arm was immersed in lukewarm water for twenty minutes; it was then withdrawn, and the surface lightly dried. Immediately after this I again plunged it in the water, and now the surface was covered with a layer of oil, and the other precautions taken, as in the former experiments. In twenty-two minutes the fluid had sunk half an inch in the tube.

The object of this experiment was to determine whether previous immersion in water, by saturating the cuticle, would cause any change in the subsequent absorption. The result proves that no such effect is produced. And this is precisely what we might have anticipated, for we can see no reason why the liquid, when once introduced into the substance of the cuticle, (by a process to be explained hereafter), should be arrested there, and obstruct the further entrance of any additional quantity, instead of being carried forward on its original course into the general mass of circulating fluids.

been composed chiefly of vegetables, and therefore altogether different from the soup I employed. Be this as it may, no one can, I think, attentively consider the facts now laid before him, without at once confessing that fluids, when applied to a part only of the surface, can readily penetrate into the interior.

Such, then, being the evidence in favour of absorption, from the *local* application of water, I now turn to consider the effects of *general* baths, a subject to which more attention has been paid, and on which are based some of the most celebrated arguments against the doctrine under review.

We have seen that the ancients placed considerable reliance on the efficacy of medicines applied to the surface, and in accordance with this belief, they were in the habit of prescribing baths of asses' milk, for the restoration of juvenility, as well as the cure of disease;[1] and it was probably with a similar intention, that Poppeia, the wife of Nero, habitually employed this luxury.[2] Avensor, a Greek physician, who was the first to describe stricture of the œsophagus, proposed the use of baths of milk, or other nutritious fluids, to support the patient's strength, until he was able to relieve the obstruction.[3] Paracelsus bears witness to the efficacy of this method;[4] and Van Mons asserts that he nourished a patient, whom a wound of the larynx had rendered incapable of swallowing, by the application of sponges soaked in broth to various parts of the body.[5] Cruickshanks relates an exceedingly interesting case of a patient labouring under stricture of the œsophagus, who, for the space of two months, was unable to receive anything, either solid or fluid, into the stomach. He was tormented with thirst, and complained much of inability to make water.

[1] *Dr. Wilkinson.* On the Power of External Absorption of the Human Body.—Med. Museum, ii. 117. 1781.

[2] *Duval.* Med. Eispnoïque, &c.

[3] *Cruickshanks.* Anatomy of the Absorbing Vessels, 4to. Lond. 1780, p. 16.

[4] *Adelon.* Diet. de Med.—"Absorption."

[5] *Westrumb.* Jour. Compl. xxx. p. 270.

But all these symptoms were effectually removed, by the employment of the warm bath for one hour, morning and evening, thirst being alleviated, and a natural quantity of urine evacuated.[1]

The testimony of numerous voyagers might be also quoted, as bearing direct evidence in favour of this doctrine, but a few examples only will suffice.

Basilowitsch has recorded the history of a sailor, who, being cast on a desert island, saved himself from dying with thirst, by bathing and washing himself frequently in the sea.[2] Captain Kennedy, in his narrative, published in Dodsley's Annual Register for 1769, thus expresses himself,—"I cannot conclude without making mention of the great advantage I derived from soaking my clothes twice a-day in salt water, and putting them on without wringing. There is one very remarkable circumstance, and worthy of notice, which was, that we daily made the *same quantity* of urine as if we had drank moderately of any liquid, which must be owing to a body of water being absorbed through the pores of the skin. So very great advantage did we derive from this practice, that the violent drought went off, the parched tongue was cured in a few minutes after bathing and washing our clothes; at the same time we found ourselves as much refreshed as if we had received some actual nourishment."[3] And, lastly, Captain Bligh's most interesting account of the sufferings undergone by himself and his gallant crew, in their perilous voyage, after the mutiny of the Bounty, contains many most convincing proofs of this fact. For while exposed to the inclemencies of a tempestuous season, to intense fatigue, and great deprivation both of food and drink, the pangs of thirst were totally averted,

[1] *Cruickshanks.* Loc. cit. p. 108.

N.B.—*Currie* does not consider this case of any authority, on what grounds I know not, since he has not deigned to favour us with his reasons for such an opinion.—Vide Med. Rep. i. 316, (note).

[2] *Westrumb.* L. c. 270.

[3] Vide *Clare.* On the Cure of Lues, &c., 12mo. Lond. 1780, p. 44, et seq.

by the simple expedient of soaking their clothes in the sea, and putting them on while wet.[1]

But valuable as these observations undoubtedly are, and the more so, as coming from men who had no theory to support, they can only be regarded in the light of corroborative testimony; the appeal must be made to experiment, before we can hope for a perfect elucidation of the truth. To these, therefore, I shall now direct my attention; and here at the outset I cannot but lament that Haller, whose genius has thrown so much light over the intricacies of physiology, should have almost totally neglected this inquiry, contenting himself only with a general assertion, that water is absorbed from the surface of the body, since in the bath the skin swells, the weight is increased, and thirst allayed.[2] What he has left undone, succeeding observers have laboured to supply; and foremost of these stands Séguin,[3] whose experiments demand the more particular notice, that they were performed with every attention to accuracy, were frequently repeated, and gave pretty uniform results; results, moreover, which are often quoted as at once conclusive of the point at issue, but which I hope to prove are far from warranting so decisive a conclusion. They were as follow:—[3]

After having performed frequent ablutions, with a view to clear the body effectually from all extraneous matter, he was carefully weighed in the air, and the process was again repeated after a certain interval; the whole loss of weight being then divided by the number of minutes during which the experiment lasted, the quotient indicated the mean loss for every minute. Immediately afterwards he entered the bath, and at the end of three or four hours was again weighed, and a similar calculation instituted. The results of thirty-three

[1] *Bligh.* Voyage to the South Sea, &c., 4to. Lond. 1792, p. 191, et seq.

[2] *Haller.* Elementa, v. l. 12, p. 88.

[3] *Séguin*, in *Fourcroy.* La Médecine éclairée par les Sciences Physiques, t. iii. p. 232, 8vo. Paris, 1791, 2; or more fully in Annales de Chimie, xe. 185.—Mémoire sur les Vaisseaux Absorbans, &c.

experiments thus performed, showed, 1st, That in no case was the weight increased during immersion. 2d, That a little less was lost in the bath than in the air, but that this diminution varied much, according to particular circumstances, especially according to the greater or less temperature of the water. 3d, That at a pressure of twenty-eight inches of mercury, the loss of weight in the water was to that in the air, in the proportion of 6·5 : 17, provided the temperature of both media was not above 10° or 12° Reaumur.[1] 4th, That under the same barometric pressure, the proportion is as 7·5 : 21·7, the temperature being from 15° to 18°, and as 13 : 23, when the temperature was so high as 26° or 28°.

A few preliminary remarks will be necessary, before entering upon the consideration of these most valuable experiments. It appears to me, that in reasoning upon all observations of this nature, too little attention has been paid to the circumstances under which the body is placed, circumstances which are of themselves peculiarly liable to produce diminution of weight, and the effects of which no previous care can totally avert. The contact of water will occasion this,—1st, by the removal of extraneous matters formerly adhering to the surface ; 2d, by the washing away of that sebaceous exudation by which the skin is generally lubricated ; and, 3d, by the effect of friction during the process of drying, by which the loose external scales of the cuticle are separated, in no inconsiderable quantities. That these objections are not theoretical, will be allowed by all who have had much experience in warm baths, and although the effects of each, when taken separately, may be small, yet in collection the diminution thus produced will not appear unworthy of notice.

But I hasten to a more important subject. Does the contact of water prevent the process of exhalation ? Keill, Cruick-

[1] N.B.—In this part of his Memoir, Séguin does not state what thermometer he employed, but in a subsequent observation he gives the degrees according to Reaumur. I presume, therefore, that he made use of the same scale throughout.

shanks, Desault, Marcard, and Bardsley, are all of opinion, that so far from being prevented, or even diminished, its quantity in the warm bath is sometimes greatly increased.[1] Thus Keill computes, that while transpiration in the air is about ℥ſs per hour, in the bath it amounts to ℥viii; and Marcard believes that ℔i may be thus lost, even without the production of sweat.[2] To illustrate the truth of his opinion, he applied a thick plaster of Burgundy pitch upon his back, but found that notwithstanding this, the aqueous exhalation accumulated below to such an extent, that after a time it could be removed with ease; and he then asks, "If this tenacious substance did not prevent exhalation, is it not still less probable that water would be able to produce such an effect?"[3] Lemonnier, after staying eight minutes in water at 113° F., lost ℥xx, which is at least double what Delaroche and Berger lost at the same temperature in a vapour bath, and at a temperature above 194° F. in dry air.[4] But the arguments of Dr. Edwards are so full and convincing upon this point, that no attentive reader can, I think, fail at once to acknowledge the accuracy of his doctrines, which are moreover supported by what we have already learned concerning the structure of the skin. He has shewn that there are two distinct modes of perspiration.[5] One by evaporation, which is a physical process, taking place equally in dead matter and in living bodies "and a consequence of that porosity of organised bodies, by which the liquids near surfaces exposed to the air would diminish in quantity, by being converted into vapour, even though the pores should be such as not to give passage to a single drop of liquid." The other by transudation, which is for the most part a vital action, "a function which appears to be always in operation, although

[1] *Wood.* On Structure and Functions of the Skin, p. 51, 8vo. Edin. 1832.

[2] *Marcard.* De la Nature et de l'Usage des Bains, pp. 171, 2.

[3] Loc. cit. p. 180.

[4] Vide *Edwards.* Loc. cit. 199.

[5] *Edwards.* L. c. 171, et seq.

varying in activity; which may be modified by external agents, but which essentially depends upon causes inherent in the living economy." These two forms, although usually combined, are yet independent of each other, and may exist separately; that in fact they do so, he has proved by direct experiment, upon numerous species of cold-blooded vertebrata. These animals, when confined in air saturated with moisture, and at the same temperature as their bodies, and when consequently no perspiration by evaporation could take place, were yet found to lose weight,—an effect which can be ascribed solely to transudation. In man, and other warm-blooded animals, when exposed to the same conditions, transudation takes place to such an extent, that the sweat streams from all parts of the body. The truth of this opinion is amply verified by the experiments of Delaroche and Berger, made in the stove and vapour bath, in which they uniformly found that air excessively hot, and charged with extreme humidity, excited a more abundant perspiration than dry air at a higher temperature.

If we now apply these facts to the consideration of the warm bath, we shall at once perceive, that while perspiration by evaporation is effectually prevented, the circumstances are precisely those which would favour transudation, and this will take place the more abundantly, according as the temperature is more elevated. Thus, in his experiments upon Batrachians, Edwards found that on comparing the total losses in the space of six hours, at the temperature of 32° F., and those which took place at 104° F., they were nearly as 1 : 55.[1] Séguin's own experiments give an analogous result, and yet he denies that transudation goes on in the bath!

Having then shewn that cutaneous exhalation is not prevented but rather increased by immersion in warm water, let us now for a moment turn our attention to the lungs. Owing to the structure of these organs, there can be no loss by trans-

[1] Loc. cit. p. 176.

udation, the whole quantity of water removed must make its exit in the form of vapour, and this explains the reason why in man the losses by cutaneous perspiration, are more abundant than those taking place from the pulmonary organs, though the internal surface of the lungs so far exceeds that of the skin. Being, therefore, a purely "physical process, it may be stopped by the physical conditions which prevent evaporation. In an atmosphere saturated with moisture, if the temperature were equal to or above that of the body, there would be no watery perspiration from the lungs, because there would be no evaporation."[1] But do these conditions exist, when the body is immersed in the warm bath? Few, I think, will answer in the affirmative. That the air contains a larger proportion of hygrometric moisture than usual, I am quite prepared to admit, but that it is by any means saturated, or that under ordinary circumstances, it is nearly of the same temperature as the body, I must at once deny; and consequently I hold it to be incorrect to assert, as some have broadly done, that pulmonary exhalation is in these cases totally suppressed; forgetting, moreover, that the whole loss by the lungs does not consist of watery vapour, but that part is also due to the chemical changes produced by respiration.

To proceed, however, with our examination of Séguin's experiments. In this author's second joint memoir with Lavoisier, he has stated that the proportion which the pulmonary bears to the general exhalation is as 7 : 18.[2] Now, in his first series of experiments, at a temperature of 60° F., the quantity lost in the bath was to that lost in the air as 6·5 : 17, shewing at a glance that here the lungs alone were concerned; and consequently since cutaneous transpiration was proceeding as before, a quantity of water sufficient to supply this loss must have been absorbed. In the second series, at a temperature between 70° and 80° F., the result is still more favourable, the proportion being as 7·5 : 21·7. But in the third the loss

[1] *Edwards.* L. c. p. 180. [2] *Séguin.* Ann. de Chimie, xc. 22.

was much greater, the proportion being as 13 : 23, a result by no means wonderful, if we consider the very elevated temperature, about 100° F., and the excessive transudation which must necessarily have followed.

Viewed in this light, then, so far from being conclusive against the doctrine we are advocating, these experiments afford convincing proof that water was absorbed. Whether this effect were produced by the skin or the lungs, many may be inclined to doubt, and those who deny the existence of cutaneous absorption will at once attribute it to the latter. Let us therefore now turn our attention to this branch of the subject. That the bronchial membrane does indeed absorb some substances with great rapidity, no one can for a moment deny. The experiments of Bichat, Klapp, and Rousseau, with vapour of turpentine ; the speedy conveyance of mercury into the system, by the respiration of its fumes ; the rapidity with which poisons thus applied produce their specific effects ; and the fact that salts have been discovered in the blood, the solutions of which had been poured in small quantities into the bronchi,[1] all bear conclusive evidence in favour of this opinion. But it is with the absorption of water that we are at present engaged, and here the evidence is essentially defective. Dr. Dill, in an admirable paper on cutaneous absorption has related two experiments, the results of which tend decidedly to contradict the idea of its existence.[2] Having accurately weighed himself, and noted his loss by exhalation, he inhaled for two hours the steam of boiling water, taking care to keep the surface of the body cool, to prevent any increase of perspiration. When he was weighed again, and the proper deductions were made, *no increase* could be detected, and consequently no absorption had taken place. The experiments which I have myself instituted, although they do not entirely confirm the observations of Dr. Dill, yet shew

[1] *Meyer.* Bibliothéque Universelle, Jan. 1818.
[2] *Dill.* Ed. Med. Chir. Trans. ii. 350.

that the quantity thus introduced into the body is very small and variable, when compared with that which other trials had convinced me takes place from the surface. They were as follow:—

Experiment IX.—At eight P.M. the weight of a young boy was ℔b72+℥x+ʒvii+gr.x. At 8+30′, it was ℔b72+℥x+ʒi+ϑii, thus indicating a loss by cutaneous and pulmonary exhalation of ʒvfs. He then inhaled the vapour of hot water during the space of half an hour, when his weight was found to be ℔b72+℥ix+ʒvii, shewing a loss of only ʒii+ϑii. It might therefore be imagined that absorption had taken place to the amount of ʒii+ϑii+gr.x, but a little consideration will render it apparent that such a conclusion would be totally incorrect. Séguin computes the exhalation from the lungs of an adult at ʒiii during the half hour; in a child of course this must be considered somewhat less, though from not being in possession of a perfect apparatus, I was unable to determine the exact amount of exhalation. Considering, however, that in the evening, the time at which the experiment was performed, according to the observations of Wood,[1] the watery exhalation of the lungs is at its maximum, while that of the skin is at its minimum, and that the whole loss observed in this instance, during the previous half hour, amounted to no more than ʒvfs, I am inclined to think that the diminished loss during the inhalation of the steam, is to be nearly, if not altogether, attributed to suppressed exhalation, an effect which might be naturally expected to occur, since all the necessary conditions above described were present.

The next experiment, however, would certainly appear to indicate, that a slight degree of absorption had taken place by the lungs.

[1] *Wood.* Loc. cit. 38.

Experiment X.—At 10 minutes before 9 P.M., the same individual weighed ℔b73+℥vi+℥fs. At 9+20′, his weight was ℔b73+℥v+℥iv, the rate of exhalation being therefore ℥ivfs in the half hour. He then, as before, inhaled for thirty minutes, at the expiration of which period he had only lost ℥i+℈i, a diminution which, as far as I am aware, can be attributed only to pulmonary absorption.

If these experiments do not prove that the lungs are incapable of absorbing watery vapour, they are yet I think sufficient to shew that the extent of that power has been greatly exaggerated; that it is very slight, extremely variable, and not by any means equal to that which some physiologists are inclined to believe, resting their opinions, however, upon pure assertions, unsupported by even the shadow of a proof, a method which is surely at variance with every principle of inductive philosophy. They forget, moreover, that by thus gratuitously conferring this power upon the lungs, they involve themselves in the difficulty of explaining why no absorption took place in these experiments of Séguin, and in others to be immediately mentioned.

But it is time that I should now advert to the experiments of the celebrated Dr. Currie of Liverpool, detailed in the first volume of his Medical Reports;[1] experiments which, no less than those of Séguin, have been frequently quoted as entirely subversive of the doctrine under review. They are as follow:—

A diabetic patient was first submitted to trial, and immersed in a bath at 96° F., but without any sensible variation in his weight being produced. He then made five experiments of a similar nature upon himself, varying the heat of the bath from 87° to 95° F., but in no instance found his weight augmented. To these experiments, however, the following objections immediately suggest themselves. Independently of the diseased state of the cuticle, which by the Doctor's own confession

[1] *Currie.* Med. Reports, i. 303. 1804.

existed in the first patient, and which may be reasonably supposed to have presented a greater obstacle to the entrance of fluids than occurs under ordinary circumstances, the experiments are of themselves essentially imperfect. For we have no account of the rate of loss which the body was undergoing previously, and which must have been either arrested during immersion in the water, or supplied from some other source, since "no variation in the weight" could be observed. We are left totally in the dark as to the time at which the trials were made, whether it was before or after the reception of food; as to the state of the circulation before and after immersion; and as to the effect produced upon the exhalation by the elevated temperature to which the skin was exposed. All the above circumstances are well known to exercise no slight degree of influence, and inattention to them would render unsatisfactory the results of any experiments, no matter how carefully they might be performed in other respects.

But the case on which the greatest stress is laid is that of the unfortunate Mr. M., a patient labouring under dysphagia in its most advanced stage, the introduction of any nutriment into the stomach, whether solid or fluid, being totally impracticable. Under these melancholy circumstances, an attempt was made to prolong his existence by the exhibition of nutritive enemata, and immersion of the body, night and morning, in a bath of milk and water. That these means should prove ineffectual will astonish no one who for a moment reflects that even supposing a sufficient quantity of the nutritious fluid to have been thus introduced into the system, it could not by any possibility have undergone that species of digestion which is essential to the formation of perfect chyle, and that great inanition must have been the inevitable consequence, even independently of the pernicious influence exerted by a malignant disease. He was weighed three times during the course of these experiments, and though at the first trial he appeared to have gained ℥xxx, this seeming increase was traced to an error in the calculations, and the two subsequent observations

shewed not the slightest change of weight. But mark the effect upon the system at large. "Thirst," says Dr. Currie,[1] "was troublesome during the first days of his abstinence, but it abated, and as he declared, was always removed by the tepid bath, in which he had the most grateful sensations." "His urine, reduced to a few ounces in the twenty-four hours, was become extremely high-coloured, and almost intolerably pungent, but in a few days it flowed without pain, and of the natural appearance, the quantity being from ℥xxiv to ℥xxxvi in twenty-four hours, more than the *whole loss of weight* in the same time."[2] At the time when the baths were employed, Mr. M. was wasting ℥xx in the twenty-four hours, and consequently during the hour of immersion he ought to have lost ℥vi + ℈ii, but the weights never varied. Either, then, absorption must have been effected, or all exhalation must have been stopped, an occurrence which, as we have already shewn, could not have taken place under any circumstances, and which was still less likely in the present instance, since the temperature of the water was 96° F., and we are informed, "that the forehead became covered with a gentle moisture,"[3] and that when laid in bed, general diaphoresis ensued. And, lastly, "It cannot be doubted," says Dr. Currie, "that the discharge by stool and perspiration exceeded the weight of the clysters,"[4] while, as before observed, the quantity of urine evacuated was of itself greater than the whole amount of loss. So far, then, from being opposed to the doctrine, these observations afford still more satisfactory and convincing proof of its truth than those of Séguin. But our evidence does not stop here,—the strongest arguments yet remain to be noticed, and to these we shall now direct our attention.

Dr. Young, in an admirable thesis published in this place in 1813, details a number of experiments, which were performed with the greatest care, and which afford the most unequivocal proof of cutaneous absorption, all chance of fallacy

[1] L. c p. 312. [2] P. 309. [3] P. 318. [4] P. 326.

from the lungs being avoided, by breathing through a tube the air external to the chamber in which the bath was situated.[1] The following is an abstract of the results. In the first and second experiments, after immersion in water at 80° F. during the space of one hour, he had actually *gained* ℥v, the pulse and heat of his body being unaffected. In a bath at 90° F., during the same period, he had gained only ℥i+℥ii, but the pulse was increased in frequency. And when the temperature of the water was at 100° F., the acceleration of the pulse was much greater, the temperature of the body was considerably augmented, and no change of weight was perceived. In all these cases, the secretion of urine was greatly increased after the bath, an additional proof that the water had made its way into the system. And it is moreover to be observed, that the *absolute gain* which took place cannot be ascribed, by even the most sceptical, to suppressed exhalation; whilst if we believe, what is probably the truth, that this secretion was still going on, the quantity absorbed must have been still greater.

No less satisfactory and conclusive are the experiments of Dr. Dill, related in the paper to which reference has been already made, although the increase observed was somewhat less. It is needless to enter into the particulars of these experiments, since they differ in no material respect from those already described; and I hasten, therefore, at once to give an account of my own observations.

The balance which I employed was extremely sensible, vibrating perceptibly with a weight of a very few grains; and in order to obtain the greatest possible accuracy in my results, I made use of the following plan. Having carefully noted the spot to which the extremity of the beam, when perfectly horizontal, pointed, this was taken as the centre of a scale, constructed with the greatest care, by adding weights alternately to the beam and the seat, and marking the respective

[1] *N. L. Young.* De Cutis Inhalatione, 8vo. Edin. 1813.

points which the beam indicated. By this means I was enabled to measure with the greatest accuracy any weight not less than gr.x, but lower than this I did not attempt to go, nor do I think that it was necessary. The scale then being affixed to the wall, and the balance so arranged, that the extremity of the beam pointed exactly to the horizontal line, I was accurately weighed, and the same process was repeated in half an hour. Immediately afterwards I entered the bath, and to avoid the most distant chance of fallacy from pulmonary absorption, my head was enveloped in an oiled cloth bag, to which a long glass tube was attached, and passed out of window, so that I breathed the external air alone. After remaining immersed for the same length of time, I was carefully dried, and again weighed. The results of twelve experiments thus performed, though not uniformly the same, were yet upon the whole extremely satisfactory.[1]

Experiment XI.—At a quarter before 7 A.M., having just risen, my weight was ℔b111+℥i+℥iv+℈ii. In an hour and a-half, that is, at a quarter past 8, I had lost ℥ii + ʒvi, giving an average expenditure of ℥vii + ℈i during the half hour.

I then entered the bath at 84° F., and remained immersed during the space of thirty minutes, all the precautions above specified being carefully observed. At the expiration of this period I had *gained* ℥fs. Add to this ʒiii for pulmonary exhalation, and the whole increase, even upon the supposition that cutaneous transpiration was completely suspended, amounted to ℥vii. The barometer stood at 29·63 in.

Experiment XII.—At 10 minutes past 8 A.M., my expenditure during the previous hour having been ʒx, I entered a bath, the temperature of which varied from 84° to

[1] NOTE.—The whole of these bath experiments were performed in August and September 1836, the weather being for the most part fine, and the air of moderate temperature.

88° F., and remained there during half an hour; when, upon being weighed, it appeared that I had lost ʒii+ϑi, thus indicating only an absorption of ϑii, to compensate for pulmonary exhalation. The barometer stood at 29·35.

The very different result observed in this case, from that which the former experiment afforded, is probably to be attributed to the fact that I was far from well, having passed a restless and feverish night, and my skin before immersion having been hot and dry.

Experiment XIII.—The height of the barometer being 29·96 in., at half-past 7 A.M., I entered the bath at 84° F., my expenditure during the preceding half hour having been ʒvi. After immersion for thirty minutes I had gained ʒi+ϑi+gr.x. The morning was clear and dry. The secretion of urine was copious after the bath, and some thirst which I had previously experienced was greatly allayed.

Experiment XIV.—At 5 minutes before 7 A.M., the barometer standing at 29·60 in., my weight was ℔110+ʒv+ʒvii+ϑi+gr.x. At 25 minutes to 8, it was ℔110+ʒv+ʒi+ϑi, thus indicating an expenditure of ʒvi+gr.x during the half hour. I then entered the bath at 84° F., and remained there during the same length of time, at the expiration of which I weighed ℔110+ʒv+ʒii+ϑi, shewing an absolute increase of ʒi. My pulse was unaltered, being 70 both before and after the bath.

Experiment XV.—At 20 minutes past 3 P.M. my weight was ℔109+ʒvi+ϑi+gr.x. At 10 minutes before 4, it was ℔109+ʒv+ʒvi, giving a loss of only ʒiiſs during the half hour. I then entered the bath at 88° F., and remained there the usual time, when I weighed ℔109+ʒv+ʒvii+ϑii+gr.x, shewing an increase of ʒi+ϑii+gr.x, independently of the quantity required to compensate for pulmonary loss. The height of the barometer was 29·8 in. I had previously

taken much exercise, and but little nourishment, since 9 A.M. Perhaps this circumstance, taken in connection with the fact of my having felt very much chilled during the half hour before the bath, may account for the very remarkable diminution of transpiration which was noticed.

Experiment XVI.—At 15 minutes past 6 P.M. my weight was ℔b111+℥xii+ʒi+℈ii. At 15 minutes before 7, it was ℔b111+℥xi+gr.x, thus indicating a loss of ℥ixfs during the half hour. I had dined heartily at five o'clock. Having then entered the bath at 88° F. for the same length of time, and being carefully dried, my weight was ℔b111+℥xi+ʒi+℈i+gr.x, shewing an absolute increase of ℥i+℈i. My pulse was 80 both before and after the bath, and the barometer stood at 29·5.

Experiment XVII.—At 20 minutes past 6 P.M., having just dined, my pulse was 82, and my weight ℔b112+℥iv+ʒvi+℈ii. I then entered the bath at 94° F., and at the expiration of half an hour my pulse was 110, and my weight ℔b112+℥iv+ʒvii+℈ii, thus shewing, even under these most unfavourable circumstances, an actual increase of ʒi. I did not observe my rate of loss, being desirous to see what would be the effect of immersion immediately after taking a full meal. The barometer stood at 29·45 in.

Experiment XVIII.—The barometer being at 29·5 in., my weight at 10 minutes past 3 P.M. was ℔b110+℥v+ʒvii. After the expiration of half an hour it was ℔b110+℥v, thus shewing an expenditure of ʒvii. My pulse was 80. After immersion for the usual time in a bath at 98° F., my weight was ℔b110+℥ii+ʒv+℈ii+gr x, indicating *a loss* of ℥ii+ʒii+gr.x. This result was evidently dependent upon the exalted temperature of the water, from the effects of which, the sweat, when I left the bath, was streaming from my face, and my whole body became rapidly bedewed with profuse per-

spiration. My pulse now beat 98. Another experiment performed at the same temperature, but into the particulars of which I shall not enter, since it was not conducted with sufficient accuracy to ensure any trustworthy results, is yet of some value as corroborating one point, viz., the considerable loss which may be occasioned, under *certain conditions of the body*, by immersion in water at so elevated a temperature. I say under certain conditions of the body, because it is well known that the same causes will not in all cases produce the same degree of perspiration.

Experiment XIX.—At a quarter past 6 P.M., having just dined, I weighed ℔b112+ʒvii. After immersion for thirty minutes in water at 91° F., my weight was increased by ℈ii. My pulse beat 80 both before and after the experiment. The height of the barometer was 29·8 in.

Experiment XX.—At 3 P.M. my weight was ℔b110+ʒv +℈ii. At thirty minutes after three, it was ℔b110+ʒiv+ ʒii, thus shewing an expenditure of ʒvi+℈ii in the half hour. Pulse 78. After immersion for the usual time in water at 92° F. my pulse was 92, and my weight ℔b110+ʒiv+ ʒii+℈ii, shewing an increase of ℈ii. The heat of the water in this instance was very oppressive, and left a severe headache. Barometer 29·65.

Experiment XXI.—At 10 minutes past 6 P.M., having just dined, my weight was ℔b110+ʒix+ʒvi+℈ii+gr.x; in half an hour it was ℔b110+ʒviii+ʒvi, shewing an expenditure of ʒi+℈ii+gr.x during the half hour. I then entered the bath at 90° F. for the usual time, when I weighed ℔b110+ʒviii+ʒvii+℈i+gr.x, shewing an absolute increase of ʒiſs. My pulse before the experiment was 80; after it, 92. Height of barometer 29·8.

Experiment XXII.—At 20 minutes before 4 P.M. my

weight was ℔b110+℥viii. At 10 minutes after 4, it was ℔b110+℥vii+℥i+℈i+gr.x. My expenditure, therefore, during the half hour was ℥vifs. Pulse 80. After immersion during the usual time in water at 90° F. I weighed ℔b110+℥vii+℥vi+℈ii+gr.x, thus indicating an absolute gain of ℥v+℈i. Pulse 82. I had previously taken much exercise, and fasted for some hours.

Such are the experiments which I have performed upon this part of our subject. Their results, though far from being uniform in *degree*, were yet remarkably so in *kind*, and I did not therefore consider it necessary to prolong my investigations upon this particular point, to the exclusion of others perhaps more important.

With a view to present these results in a compact form, and one easily examined, I have constructed the following table :—

No	Date.	Time.	Bar.	Expenditure.	Temp.	Gain.	Loss.
11	Aug. 20	8 A.M.	29·63	℥vii+℈i	84° F.	℥iv	
12	— 22	8 A.M.	29·35	℥v	84°—88° F.		℥ii+℈i
13	— 24	7½ A.M.	29·96	℥vi	84° F.	℥i+℈i+gr.x	
14	— 25	7½ A.M.	29·60	℥vi+gr.x	84 —	℥i	
15	— 26	4 P.M.	29·80	℥iifs	88 —	℥i+℈ii+grx	
16	Sept. 2	7 P.M	29·60	℥ixfs	88 —	℥i+℈i	
17	— 5	6½ P.M.	29·45	not observed	94 —	℥i	
18	— 6	4 P.M	29 5	℥vii	98 —		℥ii+℥ii+gr.x
19	— 7	6 P.M.	29 8	not observed.	91 —	℈ii	
20	— 8	3½ P.M	29·65	℥vi+℈ii	92 —	℈ii	
21	— 9	7 P.M.	29·8	℥i+℈ii+gr.x	90 —	℥ifs	
22	— 10	4 P.M.	29·9	℥vifs	90 —	℥v+℈i	

Having thus completed our examination of the effects produced by water, when applied to the surface of the body, we may now pause a moment, to consider how the question of Cutaneous Absorption stands affected by these observations. We have seen the great reliance which the opponents of the doctrine place upon certain experiments of this class, and we have also seen how insufficient they are to fulfil the purpose

designed. We have shewn reason for believing that the effects which all acknowledge to result from the bath, are not to be ascribed to suppressed exhalation, inasmuch as that suppression does not exist; and we have proved that the grand source of error, so strenuously insisted on, has been much exaggerated, that the lungs do not absorb watery vapour with the rapidity, or to the extent that many imagine. But we have further observed, that when even the most remote chance of fallacy from this source was carefully avoided, the results of numerous experiments unequivocally shewed, that the body does increase in weight during its immersion in the warm bath. The result is therefore obvious,—either the experiments are incorrect, or they afford the most conclusive evidence in favour of Cutaneous Absorption which their nature will allow.

An inspection of the table which I have constructed, and a consideration of the experiments upon which it is founded, will, however, render it at once apparent, that absorption in the bath is remarkably variable; and that, moreover, so uncertain are these variations, that it is extremely difficult, if not quite impossible, to discover any general laws by which they are regulated. The previous reception of food, at least within the limits of an ordinary meal, does not appear to exert that degree of influence which many have imagined; what effect the absolute saturation of the body might have produced, I had no opportunity of ascertaining, but we may reasonably suppose, that under such circumstances, the further introduction of fluid into the system would have been impossible. The changes observed could not, on the other hand, be attributed to the fluctuations of the barometer, nor to the greater or less excitement of the circulation, provided only that the cutaneous exhalation remained unaffected. Indeed, this is the circumstance upon which I am inclined to lay most stress, believing that Cutaneous Absorption is far more influenced by the state of this excretion than by any other condition. The truth of this opinion is, I think, supported by the

facts above related, and it will tend in some measure to explain anomalies which are otherwise unintelligible.

Be this as it may, no candid inquirer,—no one who approaches the subject unblinded by prejudice, unbiassed by preconceived notions, can, I apprehend, for a moment deny, that the evidence in favour of the absorbing power of the skin is upon these points overwhelming; that it does not rest upon hypothetical grounds, but is founded upon the observation of facts; and that although liable to various modifications of intensity, it may with as much justice be considered a function of the animal economy, as digestion, circulation, or any other process with which the progress of our knowledge has made us acquainted.

CHAPTER II.

ABSORPTION OF AQUEOUS VAPOUR FROM THE ATMOSPHERE.

The division of our subject to which we must now direct our attention, is one involved in far greater obscurity than that with which we have been hitherto engaged, and we can scarcely anticipate such satisfactory results from its investigation, more especially since from the very nature of the inquiry, an appeal to experiment is almost, if not quite impossible. I shall be therefore as brief as is compatible with clearness, though the number of curious observations collected by various authors, might well demand a more lengthened notice, and would certainly render its complete omission perfectly unwarrantable.

The absorption of aqueous vapour from the atmosphere has been frequently quoted, as affording an explanation of a remarkable phenomenon not uncommonly observed in that singular affection diabetes, viz., the great disproportion sometimes found to exist between the quantity of urine evacuated and the amount of solid and fluid nutriment taken into the body; and though part at least of this effect may with justice be attributed to the waste of the body, it is evident that in many cases, to be shortly related, such an explanation will not suffice to account for all. For a collection of some of the most remarkable instances, we are indebted to the indefatigable Haller,[1] and of these I shall without apology avail myself.

[1] *Haller.* Elementa, v. 89.

Castellus relates the history of one Maseardus, who for three years passed by urine ten times more than he drank. Hornung met with a ease where the urine doubled the quantity of fluid ingesta; and Donatus one where it was three times as great. The same author has reeorded the history of a girl, who for sixty days evaeuated sixteen eups (boecalia) of urine, during the twenty-four hours, her drink amounting to only three, and he remarks that if the whole body had been eonverted into fluid, sueh an effeet eould not have taken place. Moreover, she reeovered from the disease, without having undergone mueh emaeiation.[1] Pisani speaks of a lady who passed daily 260 oz. of urine, without either eating or drinking, a somewhat ineredible story.[2] Dolæus has seen a man drink during the night three-fourths of a pint of eider, and pass three pints of urine.[3] Livizan knew a nun, who, although tormented with thirst, had a horror of liquids, and only took two 2 ℔ of nourishment daily; her fœeal evacuations amounted to the same weight, and yet for ninety-seven days she passed 43 ℔ of urine daily.[4] Baratti relates the history of another nun who, taking little or no drink, passed in ninety-three days 3674 ℔ of urine.[5] Fothergill saw a patient who drank four pints in twenty-four hours, and passed six; and Frank speaks of a girl who, taking only 7 ℔ of nourishment, both solid and liquid, passed thirty-six pints of urine.[6] In these eases, it is quite elear that no waste of the body can aeeount for the enormous quantity of fluid passing off by the kidneys, a faet which is remarkably well illustrated by the ease of a Milanese girl, who weighed 100 ℔, and yet in sixty-four days evaeuated 1470 ℔ of urine.[7] Dr. Rollo, in his admirable treatise on diabetes, has related the history of several cases, in whieh the quantity of urine ex-

[1] *Donatus.* De Medicâ Historiâ Mirabili, libri 6, 4to. Mantuæ, 1586, chap 27, p. 261.

[2] *Haller*, l. c. 90.

[3] *Westrumb.* Jour. Complem. xxx 368.

[4] Ibid.

[5] Ibid.

[6] Ibid.

[7] *Westrumb*, p. 369.

ceeded by several pounds the amount of solid and fluid ingesta, and he explains this upon the supposition that there is an increased absorption of watery vapour, both from the skin and lungs.[1]

But the great deficiency in these, and all similar instances, consists in no notice having been taken of the rate at which the body was wasting at the same time, a most important element in all calculations of this nature. The deficiency here complained of, has been in some measure supplied by the following case related by Dr. Dill, in the paper already quoted.[2] It was that of a diabetic patient, who for five weeks passed 24 lb of urine every twenty-four hours, his ingesta during the same period amounting to 22 lb. At the commencement of the disease he weighed 145 lb, and when he died, 27 lb of loss had been sustained. But the daily excess of urine over the ingesta averaged nearly 5 lb. The difference, therefore, between the urine and ingesta during the thirty-five days of the disease, amounted to 140 lb; and if we subtract from this the 27 lb of weight which he had lost, there yet remain 113 lb of urine to be accounted for.

Dr. Percival, in the second volume of his essays, relates a case of severe lientery in a lady. Her evacuations far exceeded the liquids taken, or what could be ascribed to the dissolution of her solids. During five or six days before death, she took no nourishment, and only occasionally moistened her mouth by putting her fingers into it, after having dipped them in water; nevertheless she regularly discharged one pint of urine in the twenty-four hours.[3] And the same author quotes from Dr. Chalmers the history of a negro who was gibbeted at Charlestown in March 1779, and had nothing given to him afterwards; yet this unhappy man voided

[1] *Rollo.* Two Cases of Diabetes, &c. 2 vols. 8vo. Lond. 1798, i. 25, et seq.

[2] *Dill.* Ed. Med. Chir. Trans. ii.

[3] *Percival.* Essays, Medical and Experimental, 2 vols. 8vo. Lond. 1788, 89; ii. 267, et seq.

a large quantity of urine every morning until he died. And this effect he is inclined to attribute to the heavy dews which occur in that climate.

The last case which I shall mention, is a very remarkable one detailed by Mr. Chevalier in his lectures. It was related to him by Dr. Herholdt, physician to his excellency Baron Nicolay, Russian minister at Copenhagen, an authority upon which he places the most firm reliance.[1] It is that of a young woman who was in the habit of swallowing needles to such an extent, that 300 were at various times extracted from different parts of her body. She was afterwards affected with diabetes, and a limpid discharge from the vagina. During 151 days she drank 126 pints of fluid, chiefly water, and in that time she voided 326 pints of urine, and 529 pints of the vaginal fluid.

We have also instances of thirst being allayed, and life preserved, by the moisture of the atmosphere alone. Thus Dampierre informs us that the inhabitants of the South Sea islands habitually drink very little, and that at one time when there was a great scarcity of provisions, so that each person could only eat eight or ten spoonfuls of rice daily, many of them remained for twelve days without drinking or feeling thirst, and nevertheless they all passed the usual quantity of urine.[2] Chaussier gives an account of four quarrymen, who were inclosed in a damp cave, and subsisted fourteen days upon the moisture contained in the air alone;[3] and Rathelot has also recorded similar instances.[4] According to Haller, swine and horses in Jamaica, and the island Guanaboa, drink very little, on account, it is supposed, of the humidity of the air.[5] Sir W. Hamilton, in his account of the earthquake in Italy, 1783, mentions the case of a girl, aged 16, who remained eleven days without food, in the ruins of a

[1] *Chevalier.* Lectures, &c. pp. 243–5.

[2] *Marcard.* Loc. cit. p. 170. [3] *Collard.* Arch. Gen. xi. 74.

[4] Dict. des Sc Med.—"Inhalation."

[5] *Haller.* Elementa, L. xix. sect. 2, p. 178.

house at Oppido.[1] Dr. Wilkinson states, that at Mocha and other parts of Arabia, when the weather is so hot as to cause great thirst by day, the people sleep at night on the tops of their houses, covered only by one sheet, which is soon so wet with dew, that the body is refreshed, cooled, and preserved from thirst, even though perspiring freely.[2] And, lastly, Schenckius quotes from various authors several most singular instances of abstinence from liquids, so singular, indeed, as to be almost, if not quite impossible. Thus Lasyrtas Lasionius never drank for thirty days during summer, although he did not abstain from salt provisions, and yet he urinated. Aristotle speaks of a man who never drank during his life! And Pliny declares that a Roman knight, who when young was prohibited from the use of drink on account of anasarca, after that time was never troubled with thirst, and never drank any liquids.[3]

Many physicians, and those of no small note, have also attempted to account for the sudden re-appearance of dropsical effusions, by the absorption of water from the circum-ambient air. Thus, the celebrated De Haen, finding that his dropsical patients filled equally fast, whether they were permitted to drink liquids or not, did not hesitate to affirm that they *must* absorb from the atmosphere. "Haud ita mirabimur," says he, "hominem 150 librarum, paucos intra dies, libris 50 et pluribus graviorem fieri, et novâ paracentesi totidem aquæ libras emittere, licet toto eo tempore vix liquidi quidquam hauserit."[4] The ingenious author of the Zoonomia entertains the same belief.[5] Fodéré relates a singular case of ascites, where the fluid returned rapidly after each

[1] *Percival's* Essays, ii. 267.

[2] *Wilkinson.* Med. Museum, ii. 122, (note).

[3] *Schenckius.* Observationes Medicæ Rariores, fol. Lugd. Batav. 1644. L. iii. n. 43.

[4] *De Haen.* Ratio Medendi, pars iv. c. 3; quoted by *Kellie,* Ed. Med. and Surg. Jour. i. 170.

[5] *Darwin.* Zoonomia, i. sect. 29.

puncture, the patient at the same time not losing flesh, transpiring well, and his urine equalling the quantity of liquids taken into the stomach.[1] And Mr. Ford has recorded a very remarkable case of ovarian dropsy, in which it was observed, that during eighteen days the patient drank 692 oz. or 43 pints of fluid, and she discharged by urine and by paracentesis, 1298 oz. or 81 pints, which leaves a balance of 606 oz. or 38 pints. "The result of this," says he, "is decisive, and proves unquestionably that the body must have inhaled in that time a considerable quantity of fluid, which contributed to the accumulation of water in the ovarium."[2]

But we have other, and perhaps more convincing evidence, in the examples which are upon record, of the *actual increase of weight* which the body has been found to obtain, through the medium of a moist atmosphere. Thus De Gorter observed that under these circumstances, his body would absorb from two to six ounces during the night.[3] Keill noticed a still greater increase. "27 Decemb. Hac nocte," says he, "octodecim humoris uncias, ex aere ad se somnians corpus attraxit." The person in whom this took place was a young man, weakened from want of nourishment, but otherwise in good health.[4] Rye found the body to gain 13 oz. in moist air;[5] and Lining, in two hours, observed an increase of eight ounces and a half. His words are, "The same day, again, betwixt $2\frac{1}{4}$ and $5\frac{1}{4}$ P.M., my clothing being the same, and having no exercise, I drank betwixt 23 and 25 more oz. of punch, and the air being cooled by the clouds overspreading the heavens, the quantity of urine was greatly increased, amounting in the two-and-a-half hours to $28\frac{3}{8}$ oz.; but the perspiration was so much diminished, that the quantity of humid particles attracted by my skin exceeded the quantity

[1] *Rullier.* Dict. des Sc. Med.—"Inhalation."
[2] *Ford.* Med. Communications, ii. 130.
[3] *Haller.* Elementa, v. 89.
[4] *Edwards.* Loc. cit. 188.
[5] *Young.* Thesis, p. 19.

perspired in these two-and-a-half hours by $8\frac{1}{3}$ oz."[1] The late Dr. F. Home gives the following account of an experiment performed upon himself:—"June 6; hydrom. 43, thermom. 62. Having fatigued pretty much in the afternoon, I went to bed without supper, and was so hungry that I could not fall asleep for some time. Betwixt 11 and 12 at night, and 7 next morning, I had gained 2 oz., hydrometer and thermometer as before. During that time I had separated from my vessels, though not evacuated, 6 oz. of urine."[2] Fontana has also observed a similar effect, from walking in damp air;[3] and Dr. Jurin found an increase of 18 oz. from sleeping in a cool room, after a day's exercise and abstinence."[4] Watson, in his Chemical Essays, states that a person has been known to gain 40 lb in the space of a day. He narrates also the following very singular story:—"A lad at Newmarket, a few years ago, having been almost starved, in order that he might be reduced to a proper weight for riding a match, was weighed at 9 A.M. and again at 10 A.M., and he was found to have gained nearly 30 oz. in weight in the course of this hour, though he had only drank half a glass of wine in the interval."[5] I am indebted to my friend Mr. Carpenter for another very analogous case, which he relates upon the authority of Sir G. Hill, governor of St. Vincent. A jockey had been for some time in training for a race, in which that gentleman was much interested, and had been reduced to the proper weight. On the morning of the trial, being much oppressed with thirst, he took one cup of tea, and shortly afterwards his weight was increased 6 lb, so that he was incapacitated from riding.[6]

[1] *Lining*. Phil. Trans. xlii. p. 496; 1743.

[2] *Home*. Med. Facts and Experiments. Lond. 1759, p. 249.

[3] *Cruickshanks*. Loc. cit. p. 108.

[4] *Klapp*. Loc. cit. p. 30.

[5] *Watson*. Chemical Essays, iii. 100, 101.

[6] This case has been since published in the Edin. New Phil. Journal, July 1837, in an able essay by *Mr. Carpenter*, "On Unity of Function in Organised Bodies."

We may notice, in the next place, an experiment of Boerhaave's, in which the hand was found to absorb vapour from warm water;[1] and an ingenious speculation of Dr. Combe's, that the lymphatic temperament of the Dutch may be attributed to the great absorption of water from their very moist atmosphere.[2]

And, lastly, we have to consider the experiments performed by that celebrated physiologist, to whom we have been already so much indebted,—I mean Dr. Edwards. They are as follow:—A ring adder was placed in a vessel containing air of extreme humidity. The animal was weighed at different intervals, and found at first to have lost weight; but instead of continuing to do so, it afterwards gained more than 15 gr., not above what it had weighed originally, but above the point of diminution at which it had arrived. Several Guinea pigs were confined in humid air. It was impossible to ascertain their loss by perspiration, otherwise than by subtracting from the total loss that of the alvine and urinary evacuations of similar animals in the open air; but on comparison the average of the evacuations was found to exceed the loss of weight sustained in the humid air.[3] Mr. Bell has also remarked, that if frogs be kept in a moist situation, without having access to water in any form but in vapour, the skin is always kept moist, and the water bag filled,—an effect which can only be ascribed to absorption.[4]

Such, then, being our data, it remains for us now to draw conclusions from them. Few will, I think, deny, that in some, if not in all of the cases quoted, a quantity of aqueous vapour from the atmosphere was absorbed into the interior of the body. From what surface, then, did this absorption take place? Are we to attribute the whole to the lungs or to the

[1] *Haller.* Elementa, v. 88.

[2] *Combe.* Physiology applied to Health, 12mo. Edin. 1834.

[3] *Edwards.* Loc. cit. pp. 187, 8.

[4] *Bell.* Cyclop. of Anat. and Phys.—"Amphibia."

skin? Or may we not rather suppose that both were concerned in the process?

With regard to the lungs, it is necessary to bear in mind, that whatever may be the hygrometric state of the surrounding air, that which issues from these organs during expiration is always loaded with moisture; and a moment's reflection will shew upon what this effect depends. The air which enters the lungs is every where in contact with a membrane bedewed with a fluid; in this situation it acquires a more elevated temperature, and its capacity for moisture is proportionably increased; it follows, therefore, as a natural consequence, that a considerable quantity of the fluid will be converted into vapour, and carried off with the expired air. Reasoning, therefore, in this way, we should be inclined *a priori* to deny that absorption could take place from the lungs; but the results of experiments formerly related (pp. 54—55), would appear to shew that such an effect may sometimes be produced. How, then, are we to reconcile this contradiction? The difficulty, as it seems to me, is more apparent than real. It will be recollected that in the instances referred to, the air respired was at least of an equal temperature with the body, and completely saturated with moisture. Under such circumstances, it may be reasonably supposed, that the surface of the mucous membrane would be unable to part with any of its fluid, and the well-known power which the lungs possess of absorbing liquids renders it extremely probable that a portion would be therefore re-absorbed. But the same explanation will not apply to the cases now under consideration, where the air, however humid it might be, was yet of a lower temperature than the body, and would consequently, when admitted into the lungs, be more ready to absorb than to render up water. If the force of these arguments be allowed, then it is impossible to attribute the observed phenomena either entirely or principally to the lungs; and such being the case, the skin affords the only surface at

which the absorption could take place. It has been objected to this opinion, however, that the difference in extent between the skin and the pulmonary mucous membrane is so great, that it would be unreasonable to suppose that the former should exercise this function so much more perfectly than the latter. But those who would thus argue, forget entirely that the quantity of air brought in contact with the skin during any given period, exceeds that which enters the lung in a still greater proportion, and that consequently the objection is fatal to their own doctrine, while it tends rather to corroborate the opinion here advocated,—an opinion which is moreover supported by a fact observed by Dr. Darwin, viz., that in diabetes, benefit has been derived from covering the surface of the patient's body with oil, and thus preventing the too great action of the cutaneous lymphatics.[1]

Upon the whole, then, we are, I think, justified in concluding, that the facts now related certainly constitute additional evidence in favour of the existence of this function of the skin. But at the same time, if we consider the numerous cases in which the body loses weight, even in a moist atmosphere, and the very few instances of augmentation which have been recorded, it must be admitted, that it is under particular circumstances only that the aqueous vapour contained in the circumambient air finds its way by this course into the system.

[1] *Darwin.* On the Retrograde Motion of the Lymphatics, p. 57.

CHAPTER III.

ABSORPTION OF MIASMATA, EXHALATIONS, CONTAGION, &C.

The portion of our subject upon which we now enter differs essentially from the two preceding, inasmuch as it embraces a consideration of those obscure causes of disease and death which have so long excited the attention, and roused the ingenuity, of medical inquirers, without leading to any satisfactory conclusions. It is not my purpose, however, to occupy any time with speculations concerning the nature of miasma or contagion,—I simply take it for granted that such do exist, and limit myself to the inquiry, By what channel do they enter the body ?

The remarkable prevalence of intermittent fevers in certain localities, has naturally given rise to much discussion concerning their production, but it is now almost universally believed that they are the effect of a specific poison, emanating from the earth in these situations. Great difference of opinion, however, still exists with regard to the introduction of this poison into the body, some believing that it enters along with the respired air, others that it is received into the stomach, while others, again, are of opinion that it gains admittance through the medium of cutaneous absorption. The whole subject is involved in great obscurity, and in the present state of our knowledge it is impossible to speak with any degree of confidence ; but the following reasons would appear to lend some show of probability to the latter opinion.

1. It has been long observed, that negroes are far less liable to these diseases than Europeans, and one of their most obvious peculiarities consists in the texture of their skin, which is "thick, oily, and rank to a great degree." And it has been also noticed, that among Europeans "the thick-skinned and dark haired withstand the influence of the marsh poison much better than those of the opposite temperament."[1]

2. The well ascertained fact that the ancient Romans, who employed woollen garments, were much less influenced by the malaria than their descendants; and the decline of ague which has been lately observed, since the resumption of similar clothing.[2]

3. The immunity enjoyed by the well-coated flocks which are constantly exposed to its influence.[3]

4. The greatly increased liability to infection during the night, and when the stomach is empty, at which times, as we have already seen, cutaneous absorption is most active. Thus Rullier remarks, that few who sleep in passing over the Pontine marshes escape contagion;[4] and Dr. Home declares, that the epidemic of 1748 was chiefly caught by the troops when employed on foraging parties before breakfast.[5]

It must be confessed, however, that all these observations are susceptible of a very different explanation, and are upon the whole of but slight value for our present purpose. Nor can we attach any degree of importance to the alleged baneful effects of sleeping with aged persons, the freshness and embonpoint of butchers, or the good derived from living in stables, all which circumstances have been adduced as evidence to prove that the skin has the power of absorbing the emanations of animal substances floating in the air; although

[1] Vide *Monfalcon*. On Marshes, in Ed. Jour. of Med. Science, i. 404.

[2] *Brocchi*. On the Malaria of Rome, Edin. New Phil. Jour. 1832, 3, p. 116.

[3] Ibid.

[4] *Rullier*. Dict. des Sc. Med.—"Inhalation."

[5] *Home*. Med. Facts, p. 249.

at the same time we cannot deny, that the existence of this power has been clearly shewn by the following experiment of Bichat's. That indefatigable anatomist having observed, that after staying some time in the polluted atmosphere of the dissecting rooms, the gaseous contents of his intestines became impregnated with a similar putrid odour, provided himself with a long tube, by means of which he respired the untainted external air; but nevertheless, though his skin alone was exposed, the same effects were produced.[1]

It may be well in this place, to notice shortly a circumstance of some practical importance, though perhaps not strictly connected with the immediate subject of discussion. I allude to the very injurious effects of confining the perspiration by improper clothing, or want of cleanliness, in which case, as Dr. Combe has well remarked, "there is much reason to suppose, that its residual parts are again absorbed, and act on the system as a poison of greater or less power, according to its quantity and degree of concentration, thereby producing fever, inflammation, and even death itself; for it is established by observation, that concentrated animal effluvia form a very energetic poison." He further observes, that the fatal consequences which have repeatedly followed the use of close waterproof dresses, by sportsmen and others, may be explained upon the same principle.[2] And the accuracy of this opinion is supported by an observation of Soemmering, "Post suppressos pedum sudores, glandulas inguinales tumescere, vulgatissimum est."[3]

But if these observations are unsatisfactory and inconclusive, if they be all characterised as unimportant though interesting, far different are those to which we now direct our attention, and which comprise some of the most unequivocal proofs of the existence of this cutaneous function which have been ever brought forward.

[1] Vide Dict. des Sc. Med.—"Peau," par *Chaussier* et *Adelon*.

[2] *Combe*. Physiology, p. 70.

[3] *Soemmering*. De Morbis Vas. Absorb. 8vo. Traject. 1795, p. 178.

Contagious diseases have been divided into,—1. Those which act only by positive contact, or inoculation. 2. Those which may be transmitted by the atmosphere, although communicable by contact.[1] It is with the first of these that we have most concern, although the second will afford some apposite examples. To begin therefore with syphilis, which merits especial attention, inasmuch as its pre-eminently contagious nature is undisputed. The effects which follow the application of the venereal virus to a secreting surface, or one deprived of cuticle, are too well known to require any description here; but the numerous cases now on record, render it certain, notwithstanding all that has been said by Astruc, and many other distinguished writers upon this disease, that the same effects *may* and frequently *do* follow its application to the sound skin. Thus, in the third volume of the Edinburgh Medical Essays, we have an account of several ladies, whose nipples, and other parts of the body, were infected, in consequence of having their breasts drawn by a woman who had an ulcer in her mouth; and numerous other instances of the same kind might be quoted.[2] Bell, writing in 1793, says, "A very violent form of lues broke out some time ago in Canada, and was so very infectious, as to be communicated by eating or drinking out of the same vessel, or drying with the same cloths, that had been used by those labouring under it."[3] At the same time he positively asserts, that cases have come under his notice, where the disease has been produced by absorption from the surface of the skin, where neither wound nor ulcer existed, and where both the cuticle and cutis remained entire. He believes that scarcely any part of the skin is so thick as to prevent it, more especially if the parts have been rendered tender or irritable, either by inflammation or any other cause; but that the same result may occur,

[1] Vide Cyclop. of Pract. Med.—"Contagion," by *Dr. Brown.*

[2] Vide *Adams.* On Morbid Poisons, 8vo. Lond. 1795, p. 63, et seq.

[3] *Bell.* On Gonorrhœa Virulenta and Lues Venerea, 8vo. Edin. 1793, i. 39.

when no such previous affection has been noticed. Thus, "in two instances, buboes occurred in the axillæ, and the patients were poxed by the matter of venereal sores applied to the fingers, where the skin was sound; and in another, the disease was communicated by the patient wearing the same breeches which he had used three months before, when labouring under extensive venereal sores of the penis and scrotum, but of which he had been entirely cured."[1]

The great John Hunter held precisely similar opinions;[2] and Bacot, in his Essays on Syphilis, admits the same fact.[3] And, lastly, Lagneau states that M. Cullerier, first surgeon to the Hôpital des Vénériens at Paris, has recorded many very remarkable cases, and among others, "celui d'une femme, qui, par des raisons particulières, ayant exigé, que l'éjaculation se fit sur le ventre, contracta une énorme chancre à l'ombilic, la personne avec laquelle elle communiquait étant infecté."[4]

It would be easy, but needless, to multiply examples of this nature, and equally unnecessary is it to bring forward instances of the communication of itch or porrigo, by immediate contact; cases in which the absorbing power of the skin is most apparent, but which are too well known to demand any particular notice. I turn, however, to a subject of the greatest interest, namely, the transference of disease from the lower animals to man, and the danger resulting from the contact of dead or diseased animal matter, though I must be very brief upon both these topics.

In an essay by Remer, contained in a number of Hufeland's Journal,[5] many examples are recorded of the production of glanders in man, by the simple contact of the contagious matter discharged from the nostrils of the affected horses, (a fact which has been also proved by the researches

[1] Ibid. vol. ii. pp. 6—8.
[2] *Hunter.* On the Venereal.
[3] *Bacot.* Lond. Med. Gaz. ii. 481.
[4] *Lagneau.* Sur la Maladie Vénérienne, 8vo. Paris, 1815, note, p. 71.
[5] Vide Arch. Gen. de Med. iii. 127.

of Dr. Elliotson),[1] of the transference, in a similar way, of "la plique des bêtes à poil," and the fatal effects of handling the spleen of cows affected with gangrenous inflammation. Dr. Christison has alluded to several cases, in which persons have suffered severely, and even lost their lives, from touching the skin, entrails, blood, or other parts of animals destroyed by that intractable malady, the "pustule maligne."[2] Haller quotes from Bergius the case of a medical man, who, from inserting his arm into the gullet of an animal which had died of cow-pox, expired after great suffering.[3] Dr. Hacket has recorded the history of a Mr. Guy, who, while in attendance upon a case of labour, was obliged to introduce his hand into the uterus, and in three days afterwards was attacked with violent inflammation of that arm, and the usual constitutional symptoms attending the absorption of diseased animal matter. The absorbents could be traced by the eye along the surface of the arm to the axilla, the glands of which were indurated and painful; but no wound could be detected, though the parts were carefully examined.[4] And, lastly, in the Dublin Journal of Medical and Chemical Science, for September 1837, p. 142, is related the case of a strong peasant girl, who thrust the forefinger of her right hand, which was entirely free from any injuries, into the throat of a goose which had dropped down suddenly, in order to convince herself if any foreign body might be contained therein, which was choking the animal. Soon after this she felt violent darting pains in the finger, which rapidly extended over the whole hand, and were accompanied by great swelling. A surgeon, who was summoned three days after, found the hand and the whole of the right arm much swollen, and doughy to the feel; the index finger was of an enormous size, the skin of a bluish red colour, and discharging much acrid pus. The constitutional

[1] Vide Cyclop. of Pract. Med.—"Contagion."

[2] *Christison.* On Poisons, 3d edit. 1836, p. 581.

[3] *Haller.* Elementa, v. 88.

[4] Vide Lancet for March 7, 1835.

symptoms were precisely those which accompany poisoned wounds.

The only other disease of this division which I shall notice is cow-pox; and here we are informed that many ascribe it to transference of the matter of grease from the heels of horses to the udder of the cow, by the hands of the attendants; and it is certain that the people who have charge of such cattle are themselves liable to a similar disease. Dr. Sonderland of Barmen has, moreover, asserted, "that if a woollen bed cover or cloth, which has been several days on the bed of a small-pox patient, who has either died in the suppurative stage, or is under an intense form and degree of the disease, and is confined in a small imperfectly ventilated apartment, and which has consequently been well impregnated with the poisonous principle of small-pox, be applied firmly for 24 hours to the back of one or more cows successively, the animals in a few days become sick and feverish, and the skin, especially that of the udder, is occupied by an eruption of pustules, which assume the characteristic appearance of cow-pox, and are filled with lymph, and that these will communicate the true disease."[1]

We come now to the consideration of the second class of contagious diseases, or those which, though frequently transmitted by the atmosphere, may yet be communicated by direct contact. And first of these we shall notice the plague; for although the contagious nature of this dreadful scourge has been denied by many authors, the facts on which their opinion rests appear to me quite inconclusive, while those advanced by the opposite party are so full, so numerous, and so unequivocal, as to excite our wonder that a doubt should for a moment exist. And be it observed, that the bold experiment of the late illustrious Desgenettes, who, in order to prevent panic from spreading among the French soldiery, inoculated himself with the matter of a pestilential bubo,

[1] *Craigie.* Elements of Practice of Medicine, 8vo. Edin. 1836, i. 521.

without any ulterior effects, was no more conclusive of the point in question, than the filthy mode adopted by some, of swallowing the matters rejected by vomiting from the stomachs of cholera patients, could prove that disease to be devoid of any contagious power. Assuming, then, that the plague may spread by contagion, the following reasons appear to warrant the idea, that cutaneous absorption is the method by which it enters the body.

Dr. Mackenzie, who had resided long at Constantinople, states that the bubo is always found in that limb which has touched the infectious matter.[1] Volney and others remark, that the water-bearers of Cairo are very rarely attacked, owing to their constant ablutions.[2] Dr. Wittman says that the disease never affects oil sellers and tanners. Mr. Eton (Survey of the Turkish Empire), declares that plague is unknown to those nations who are accustomed to rub themselves with oil. Jackson makes similar remarks upon the immunity of the coolies or porters of Tunis, whose bodies are constantly smeared with oil; and during the great plague of London the tallow-chandlers appear to have been equally exempt.[3] These facts, taken in connection with the results of direct experiments performed by Father Luigi and others, and the observation of twenty-two Venetian seamen, who in 1793 resided for twenty-five days with three pestiferous patients, without contracting the disease, and of three Armenian families, who, under similar circumstances, likewise escaped infection, by the simple expedient of anointing themselves with oil,[4]—all tend to prove the great value of this agent as a prophylactic; and it is evident that the only way in which it can act is by preventing cutaneous absorption, an effect which we have already shewn to be produced by this means.

[1] *Cruickshanks.* Anat. of Absorb. p. 123.

[2] *Craigie.* L. c. 376.

[3] *Hunter.* On the External Use of Oil, in Edin. Med. and Surg. Jour. ii. 185, et seq.

[4] *Craigie.* L. c.

Of small-pox, the only other disease of this class which I shall mention, it has been affirmed, that the matter taken from the pustules, and applied to the sound skin, will, under some circumstances, produce the disease. Wrisberg relates, that although in early life he had a very severe attack, yet as often as in making injections, he either touched, turned, or held the bodies of persons who had died of this malady, when immersed in the warm bath, so often did three, or four, or even six genuine pustules appear on his arms, and pass regularly through all their stages.[1]

These facts afford a sufficient answer to some of the objections urged by Rousseau, a most determined opponent of the doctrine we are advocating, viz., the safety with which anatomists dissect putrid bodies, as long as the cuticle remains uninjured, and the immunity from disease which surgeons enjoy, even when their hands are contaminated with the matter of venereal or gangrenous ulcers;[2] since they demonstrate that such results are by no means uniform, and that the most serious and even fatal effects have arisen from these very causes. But he further urges, that even the venom of the viper may be laid on the skin, without any accident ensuing. A most unfortunate illustration, since, reasoning in the same manner, we might also deny the absorbing power of the stomach, of the conjunctiva of the eyes, or the denuded skin, when we are informed by the experiments, both of Mead[3] and Fontana,[4] that the same poison may be placed upon the tongue or swallowed, without causing the slightest injury; that it produces no effect, when dropped into the eyes; and that when applied to the true skin, or inserted into small wounds of its substance, the action is only local; while, on the other hand, death ensues in a few seconds after its injection into the blood.

[1] Ibid. p. 566.

[2] Vide *Stock's* Account of, in Ed. Med. and Surg. Jour. ii. 11.

[3] *Mead.* Essay on Poisons, 8vo. Lond. 1747, p. 22.

[4] *Fontana.* On the Venom of the Viper, &c. i. 55—59.

CHAPTER IV.

ABSORPTION OF MEDICINAL AGENTS.

The division upon which we now enter constitutes, in my opinion, by far the most important section of our task. Errors may creep into the nicest calculations, more especially when, as occurred in our bath experiments, the original weights are large, while the variations are slight; and the evidence drawn from the contagion of disease will be always cavilled at, until we have discovered something more regarding its nature. But when the application of a drug to the exterior of the body is followed by the usual physiological and therapeutic effects; nay more, when its presence may be detected in the blood, or the excretions, no shadow of a doubt can remain, that the substance so applied has been absorbed into the system. That such is the case I trust to prove in the following pages, and to shew, moreover, that the effects observed cannot be attributed to pulmonary absorption. This source of fallacy has been much insisted upon, but it is evidently applicable to those drugs only which are extremely volatile, or are administered in vapour, though Rousseau has gone to the absurd length of asserting, that even in frictions with mercurial ointment, the metal is volatilised, and thus enters the body; forgetting that if such were the case, the unfortunate individual who performed the frictions would be far more readily salivated than the patient,—a result which we know from ample experience does not take place. But I hasten to commence, without

further preliminaries; and according to my original plan, I shall treat, 1st, Of the absorption of solids; 2d, Of liquids; and, 3d, Of gases and vapours,—this method being adopted, because, while it undoubtedly renders a certain degree of repetition necessary, it avoids the far more dangerous error of confusion.

SECTION I.

Absorption of Solids.

Under this head I include the consideration of the effects produced by powders, ointments, solid extracts, plasters, and poultices, both when simply applied to the common integuments, and when administered in frictions; the latter being noticed, not so much in support of the doctrine in question, as with a view to its practical application in the treatment of disease. First, then,

Of Mercury.—It is needless to occupy any time or space in adverting to the employment of this powerful remedy in frictions, since every practitioner is well acquainted with it, and knows how speedily and effectually the system may be brought under its influence by this means. But we have evidence to prove that the simple application is in some cases sufficient. Thus Haller quotes the authority of Hild in support of the fact, that mercurial ointment, smeared on the skin, may be absorbed without friction, and excite salivation.[1] Kellie affirms that the same effects may be produced by

[1] *Haller.* Elementa, v. 85.

wearing a mercurial plaster.[1] Johannes de Vigo has treated syphilis with success by the same means.[2] Neumann has been very successful in his treatment of cynanche parotidœa, by means of a plaster composed of hydriod. potass. pt. i. to ungt. hydrarg. pt. viii., the cure invariably taking place in three or four days.[3] And Dr. Colles, in his late work upon the venereal disease, has advised that while a patient is undergoing a course of mercurial frictions, the same drawers should always be worn, so that some portion of the ointment may be constantly applied to the surface, and thus absorbed.[4] Degner has adverted to examples of violent salivation, produced by the external application of a small quantity of corrosive sublimate, and gives the detailed account of a lady who lost her life from wearing a plaster which contained this potent drug.[5] And Séguin has related several direct experiments, performed by himself, of which the following is an abstract.[6] He applied to each arm of a patient labouring under the venereal disease 4½ gr. of cor. subl., covered by small pieces of plaster, which adhered only by their edges. The experiment lasted during five days, and the application was renewed daily. After the second application an eschar was formed, and the patient began to complain of his gums; the other applications were made near the injured part, and by the end of the fifth day the salivation was complete. Similar trials with calomel produced no marks of absorption. To another venereal patient 24 gr. of sal alembroth (præcip. alb.) were applied, and covered in the usual manner, the spot being changed every

[1] *Kellie.* Ed. Med. and Surg. Jour. ii. 186.

[2] *Young.* Thesis, &c. p. 14.

[3] *Neumann.* Arch. Gen. de Med. xvii. 607,—extracted from *Rust's* Magazin, 1826.

[4] *Colles.* Pract. Observ. on the Venereal Disease, 8vo. Lond. 1837, p. 41.

[5] *Degner.* De Morte per Mercurium Sublim. Emplast. applic. inductâ, 8vo. Traj. Rhen. 1754, p. 346, et seq.

[6] *Séguin.* Ann. de Chimie, xcii. p. 38, et seq.

day, on account of the formation of pustules. On the sixth day salivation commenced, and by the ninth was very profuse. A repetition of the experiment gave a similar result. But the last that we shall notice, and the most singular, is the following. To the previously well washed abdomen of another patient, he applied one drachm of calomel, of scammony, of gamboge, of sal alembroth, and of tartar emetic, each quantity being confined under a watch-glass, and retained in its place by a bandage. The patient lay on his back, and the bed clothes were supported so as not to touch him. The experiment lasted 10½ hours. The substances being then cautiously removed, replaced in their former coverings, and weighed, it was found that the calomel had lost ⅔ gr., the scammony had *gained* ⅞ gr., the gamboge had lost nearly gr.i, the sal. alemb. gr.x, and the tart. ant. gr.v. The diminution in these two last, he believes must be attributed to absorption, but thinks the other changes might be owing to unavoidable errors in the manipulation. A number of pustules had formed on the spot where the sal alemb. was applied, but the skin was quite unaltered under the tartar emetic.

The result of this very curious experiment, which is the more valuable, as having been performed by one who was much inclined to doubt the existence of the cutaneous function for which we are contending, shews in the most convincing manner, that certain substances may be absorbed, when applied in a solid form to the sound skin; and that, moreover, without the production of any peculiar *local* action. It is of the greatest moment to note particularly this latter circumstance, since it has been strongly urged by our opponents, that *all* the cases of absorption from the application of medicinal agents to the surface, have been accompanied with local action, and that without this, no such effect would have been observed. The experiment now before us is of itself sufficient to shew how unfounded is this opinion; and numerous corroborative instances will be brought forward as we advance in our inquiry. But independently of this, were the facts even

such as have been stated, the objection would not be fatal, since the very production of local excitement proves that the remedy had penetrated within the cuticle, (the only acknowledged obstacle to absorption), and made an impression on the nerves and vessels of the true skin, its further passage into the interior being then prevented by the over-action thus excited. Were not this the case, should we not more frequently see strangury occasioned by the employment of cantharides? In fact, the only objection which can be made to this explanation is, that the structure of the cuticle may be altered by some chemical change, and thus allow the substance to pass through; but it must be observed that such has never been *proved* to take place, while the well-known composition of the organ renders it *a priori* extremely improbable, nay, even impossible, in the particular case now before us.

Of Iodine, the substance which we shall next notice, but little can be said in this place, since in the solid form, as far as I am acquainted, it has only been employed in frictions, and its great value as a remedial agent, when thus administered, is allowed on all hands. But I may merely state, that besides the evidence derived from the cure of disease, we have this additional proof of its absorption, that Bennerscheidt has detected its presence in the crassamentum of the blood of a patient who had been using the iodine ointment.[1]

Of Antimony.—We have already seen that in Séguin's experiment, tartar emetic was pretty rapidly absorbed, when simply applied to the skin in the form of powder, and we have the following reasons for believing, that under certain circumstances, the application may be followed by the usual constitutional effects, though it must be allowed that in the majority of instances a local action is alone produced. Dr. Barton states that he has known tart. ant., when rubbed on the skin, produce nausea and vomiting.[2] M. Peysson has effected the cure of intermittent fever, by the employment of the ungt. ant.

[1] *Christison.* L. c. p. 14.

[2] *Klapp.* L. c. p. 34.

tart., the place of application being constantly changed, so as to avoid the chance of producing pustules.[1] Mr. King has related two cases, in which purgation was caused, by rubbing the same along the course of the spine.[2] A remarkable case is quoted in the Arch. Gen., from the Journal de Chimie Med., where most violent constitutional effects followed the application of a very small quantity of the ungt. ant. tart. to the hypogastrium, with the intention of relieving a severe pain of the bladder.[3] And, lastly, in one case death seems to have been produced by the same means,—an infant, two years old, having been seized with great sickness and frequent fainting, which proved fatal in forty-eight hours, soon after having the spine rubbed with this ointment.[4]

Of Arsenic.—There are several cases on record, in which this virulent poison has produced serious, and even fatal mischief, when applied to the common integuments. Thus Desgranges relates an instance of severe erysipelas of the head and face, ulceration of the scalp, and violent constitutional disturbance, from the employment of an arsenical ointment to destroy lice. Schulze has recorded five cases of a similar nature, arising from arsenic having been employed instead of hair powder, one of which proved fatal. And the same cause has been found by other observers to produce the same effect.[5]

Of Lead.—The instances of poisoning from the external application of this metal, and its preparations in the solid form, are few in number, and by no means satisfactory. Haller quotes from Zeller the history of a patient, in whom dyspnœa, fainting, nausea, and vomiting were occasioned from litharge sprinkled in the axilla, as a cure for redness of the

[1] *Peysson.* Arch. Gen. de Med. iii. 453.

[2] *King.* Lond. Med. Repos. xx. 148.

[3] Arch. Gen. de Med. xviii. 442.

[4] My friend Dr. Duncan informs me that he once saw violent sickness and vomiting produced by rubbing this ointment on the chest of a phthisical patient. It was consequently omitted for a time, but again resumed at two different intervals, and each time with precisely the same effect.

[4] *Christison.* L. c. p. 452.

[5] Ibid. L. c. p. 303.

face.[1] Compositors in printing offices are liable to a partial paralysis of the hands and arms; and Dr. A. T. Thomson has heard of a man, who was in the daily custom of handling pigs of lead, and loading carts with them, having twice suffered from this disease.[2] And, lastly, symptoms resembling lead colic were produced in some of Captain Back's men, while engaged in forming a red clay into mortar with sand, during their winter residence at Fort Reliance. The precise nature of the soil was not ascertained, but from the well-known abundance of lead in the northern districts of America, it is a fair presumption, that in this instance the same metal was the cause of the peculiar affection observed.[3]

Of Purgatives.—We have sufficient proofs that the bowels may be moved by the application of substances of this class to the sound skin, in the solid form. Thus, Haller affirms that bitter drugs applied externally to the abdomen, have caused the expulsion of worms; pills placed on the epigastrium have purged; and from handling colocynth, the bowels have been opened.[4] Chiarenti has produced abundant evacuations, by means of frictions with rhubarb, incorporated with lard or saliva.[5] MM. Saclier and Bretonneau have observed, that frictions with scammony upon the legs of a nurse rendered her milk purgative.[6] Dr. A. T. Thomson asserts that jalap united with lard, and rubbed on the skin, causes severe griping, though when injected into the veins it produces no effect; and that the bowels of children may be readily affected by rhubarb, applied to the abdomen in the form of poultice.[7] Chrestien has produced copious alvine evacuations in two

[1] *Haller.* Elementa, v. 86.
[2] *Thomson.* Elements of Mat. Med. 1st edit. ii. 71.
[3] Vide a paper by *Dr. Farr,* in Lond. Med. Gaz. July 9, 1836.
[4] *Haller.* L. c. 87.
[5] *Wallace* Researches on Chlorine, 11.
[6] Mém. de la Société d'Emulation, i. 522.
[7] *Thomson.* Mat. Med. ii. 291, et seq.

cases of mania, by frictions with colocynth.[1] Dr. Wilkinson states that a plaster of bullock's gall is frequently used in Italy as an anthelmintic.[2] And Alibert has related the case of a female who, after delivery, was troubled with constipation and extreme irritability of stomach. Her bowels had not been opened for five days, when an ointment containing pulv. rhoei, ʒi, pulv. jalap. gr. xii, incorporated with saliva and axunge, was rubbed on the abdomen, and next day copious purgation ensued. In two days the constipation returned, and ʒii of rhubarb were applied as before, but not with equal success. Next day pulv. scammon. gr. xii, colocynth, gr. xii, and calomel, gr. vi, were applied in like manner,—the patient herself was not purged, but her infant, whose bowels had also been constipated, was severely affected. From some experiments instituted at the Salpêtrière, it appeared that three children about five years old, and labouring under engorgements of the abdominal viscera, were abundantly purged by frictions with rhubarb and scammony, united with the gastric juice of a screech owl; and in his own person M. Alibert produced colics, gripes, and headache, by frictions with jalap, colocynth, and calomel.[3]

The result of the following experiment, performed by myself, is strongly corroborative of the above testimony, and is more valuable, as shewing that friction is by no means necessary to produce the desired effect.

Experiment XXIII.—July 19, 1836. At 12 P.M. I applied to my abdomen a large poultice of oatmeal, mixed with about ʒvi of jalap in powder. It excited such intolerable itching, that I could not sleep, and at 5 A.M. I removed it. Notwithstanding this, however, my bowels were very freely opened next morning.

[1] *Chrestien.* De la Méthode Itraleptique, p. 190. 8vo. Paris, 1811.

[2] *Wilkinson.* Med. Museum, ii. 120.

[3] *Alibert.* Mém. de la Soc. d'Emulation, i. 180.

Of Diuretics.—The efficacy of diuretics administered by the skin is established by the following observations. Chiarenti has excited copious urination in a dog, by frictions with squill dissolved in gastric juice, and Mascagni bears testimony to the good effects resulting from this practice.[1] Alibert gives the case of a child labouring under hydrothorax, in whom abundant diuresis was produced by a similar application. In another affected with ascites, the cure was accomplished without the gastric juice; but he failed in the instances of two women with anasarca of the lower extremities.[2] I have myself tried the same application without any success, in an obstinate case of hydrothorax and anasarca, in which, however, all the ordinary methods had failed, and which ultimately proved fatal.

Rogery has found powdered digitalis, incorporated with saliva, and rubbed on the inside of the arms, legs, and thighs, to act as a diuretic.[3] MM. Giulio and Rossi have experienced the best effects from this method of employing digitalis in hydrothorax, and they quote the opinion of Murray and Ray, in testimony of the advantage which it possesses over the ordinary method, from being more safe, while it is not less certain.[4] Brera has confirmed all these results, but found no benefit when oil or mucilage was employed as the menstruum. In one case the attendant who performed the frictions became similarly affected; and he mentions Dr. Ballerini of Pavia as having met with like success.[5] In a case of ascites arising from diseased heart, I made trial of a similar plan, viz., the application of an oatmeal poultice, mixed up with a strong infusion of the fresh leaves of digitalis, and certainly with the effect of increasing the quantity

[1] Vide *Brera*, in Recueil Périodique, iii. pp. 85 and 90.

[2] *Alibert.* L. c. 180, et seq.

[3] Vide *Wallace.* L. c. p. 12.

[4] *Giulio et Rossi.* Jour. de Physique, part ii. pp. 213—215, for Ann. 6. (1798.)

[5] *Brera.* Recueil Périod. iii. 85—87.

of urine; but such violent nausea and sinking were produced, that the patient refused to persevere.

Dr. Copland affirms that various substances, especially those of vegetable origin, are more diuretic when applied in the form of poultice, fomentation, or liniment, to the cutaneous surface, than when taken into the stomach, probably owing to the alteration or digestion which they undergo in the alimentary canal, by which they partially lose their activity.[1] Dr. Shortt has related several remarkable cases of dropsy, cured by poultices of the marchantia hemispherica, most extraordinary diuresis being produced.[2] I tried the same application in a case of anasarca during phthisis, not of course with any hope of effecting a cure, but simply to palliate this symptom. My expectation was, however, disappointed, for although at first the urine was increased by 14 oz., and this maintained for some days, it gradually diminished again, and the patient was not benefitted.

In the last place, Dr. Stevenson has recorded some instances, shewing that the lichen vulgaris applied in the form of poultice, over the region of the kidneys, very speedily and greatly excites those organs. Thus, in one case, the patient voided a quart every two hours, and in ten days his belly was reduced in circumference by one foot.[3]

Of Tonics.—These remedies, in the solid form, have not been much used externally; but Dr. Barton states that he has cured intermittents by cinchona poultices.[4] M. Louyer Villermay has effected the same by frictions with sulphate of quinine;[5] and Alibert has related several striking cases of the same nature, of which the following is the most remarkable.[6] A young girl, aged 14, had laboured under a double quartan for three months. After two frictions the slight

[1] *Copland.* Dict. of Pract. Med.—"Dropsy." p. 626.
[2] *Shortt.* Ed. Med. and Surg. Jour. xxxix. 129.
[3] *Stevenson.* Trans. of Med. and Phys. Soc. of Calcutta, vol. v.
[4] Vide *Klapp.* L. c. 35. [5] Arch. Gen. de Med. xii. 133.
[6] *Alibert.* L. c.

access disappeared, but the principal one returned with the same severity. After five frictions there was no more shivering, and the hot stage was much alleviated. The cure was then progressive. Dr. Pye informs us that he has cured intermittents by finely powdered cinchona placed between the layers of a double waistcoat, and worn next the skin.[1] And Dr. Wilkinson asserts, that "saffron applied to the stomach enlivens the spirits, and like a cordial exhilarates the drooping heart."[2]

We now turn to a consideration of *Narcotics*, a most important class of remedies, and one which will afford us many conclusive examples.

First, then, of *Opium*. According to Boerhaave, opium in the form of plaster has caused delirium and coma.[3] Chiarenti has remarked narcotic effects produced by the same, incorporated with gastric juice or saliva; and Richard has obtained a similar result from frictions with opium, almond oil, and spermaceti.[4] Chrestien has known pain of the epigastrium relieved by an opium plaster.[5] Chiarugi of Florence, in a letter to Dr. Frank, details several cases of mania, in which sleep was procured by the application of an ointment containing opium;[6] and Dr. Traill, in his clinical lectures, states that he has noticed the same fact.

Sir B. Brodie has found the opium plaster useful in neuralgic complaints.[7] Dr. G. Gregory recommends the same application in calculous disorders, accompanied with much pain in the loins.[8] Margot relates three cases of gastralgia, in which the greatest relief was obtained from the application

[1] *Pye.* Med. Obs. and Enquiries, ii. 245.

[2] *Wilkinson.* Med. Mus. ii. 119.

[3] *Haller.* L. c. 87.

[4] *Wallace.* L. c. 13.

[5] *Chrestien.* L. c. 104.

[6] Annals of Med. iii. 191,—a review of *Brera* on Frictions.

[7] *Brodie.* Lectures on certain Nervous Affections, 8vo. Lond. 1837, p. 27.

[8] *Gregory.* Elements of Med. 4th edit. Lond. 1835, p. 576.

of a theriac plaster, sprinkled with gr.v or vi of acet. morph.[1] And M. Lemolt states that he has experienced the most decided benefit from an application which he terms "epithème antiplogistique," and which consists of opium, saffron, mur. ammon., nitre, camphor, iron filings, siroffle, mustard, and musk. These substances, finely powdered, are enclosed in a bag, and worn applied to the epigastrium; and by this means he professes to cure all symptoms of dyspepsia. It is proper, however, to add, that in other hands this method has failed.[2]

Of Belladonna.—The rapidity with which this substance, applied to the skin, may be absorbed, is at once shewn by the dilatation of the pupil which it causes, when the extract is smeared round the eye-lids, and which is so well known, as to require little notice here; but the following cases may be stated, as shewing its further efficacy in the treatment of various diseases. Mr. Blackett has found the extract, combined with twice its weight of mercurial ointment, and rubbed along the urethra, very efficacious in relieving chordee.[3] Mr. Chevalier has employed the extract, united with cerat. saponis, in a case of severe pain of the kidneys, accompanying stricture of the urethra, with the best effect; and he finds the belladonna ointment exceedingly useful in scrofulous inflammations of glands, in periostitis, nodes, &c. A severe case of diseased knee-joint was completely relieved by the same in plaster; and this method afforded the greatest comfort to a patient labouring under ulceration of the kidneys. He has seen good result from it also in various spasmodic and inflammatory diseases of the thoracic viscera. Sometimes when the patient was very weak, the pupil became affected, and the sight grew dim; and in one case a large belladonna plaster on the loins produced a degree of paralysis of the levator palpebræ superioris, which continued many weeks afterwards, and

[1] *Margot.* Arch. Gen. de Med. xiii. 404.

[2] Vide Report by *M. Bally*, Arch. Gen. xviii. 455.

[3] *Blackett.* Lond. Med. Repository, xix. 451.

only got well under the repeated application of blisters to the temples. On the whole, he has applied this remedy to between 200 and 300 patients, in none of whom did it entirely fail.[1]

Dr. Henry has related two cases of neuralgia, cured by frictions with the extract, in one of which dilatation of the pupil was produced.[2]

In cases of colic from constriction of a part of the intestinal canal, Dr. Copland has seen advantage derived from a plaster consisting of the emplast. picis comp., the empl. ammon. c. hydrarg., and either the extract. bellad. or conii, kept long applied over the abdomen.[3] M. Ranque, of the Hotel Dieu at Orleans, has treated lead colic successfully by the following plan, viz., the application to the abdomen and loins, of a large plaster, containing hemlock, camphor, sulphur, and tart. ant. (which was removed when pustules appeared), frictions with belladonna, cherry-laurel water, and sulphuric ether, and occasionally anodyne enemata.[4]

Sir B. Brodie strongly recommends the belladona plaster in neuralgia.[5] Dr. Gregory has found hepatalgia relieved by the same means.[6] And, lastly, its relaxing properties have been successfully employed to favour the reduction of strangulated herniæ, a plan first introduced by Guerin, and afterwards adopted by several, chiefly foreign, practitioners. Thus Magliari has recorded a case, which yielded after two applications of a belladonna ointment.[7] Frankel made use of the same in two cases of femoral hernia, one being of eight day's standing, with like success.[8] Pietro Porta of San Zenone, and M.

[1] *Chevalier.* Lond. Med. and Phys. Jour. Nov. 1826, p. 403.
[2] *Henry.* Lond. Med. Repository and Review, i. 63. 1825.
[3] *Copland.* Dict.—art. "Colic."
[4] *Ranque.* Arch. Gen. de Med. vii. 379.
[5] *Brodie.* On Nervous Affections, p. 27.
[6] *Gregory.* L. c. p. 516.
[7] *Magliari.* Arch. Gen. de Med. xviii. 296.
[8] *Frankel.* Journal des Chirurgerie und Augen-Heilkunde. Translated in Lancet, March 7, 1835.

Gerard, have each also cured two cases in the same manner; and the last named author relates four cases of retention of urine relieved by frictions with the ointment on the hypogastrium or perinæum.[1]

Dr. Sigmond speaks very favourably of the external use of *Hyosciamus*, especially under the form of poultice, to relieve the pain accompanying various tumours.[2]

The well-known effect of *Tobacco* in producing vomiting, when applied as a poultice to the skin, requires little notice, more especially as the same substance will come before us again; but I may remark here, that Mr. Stedman professes to have cured liver complaints by this means, and intermittents by the use of *groundsel* poultices, which also caused severe vomiting.[3]

Nux Vomica, when applied to the skin, is generally harmless, but the following case will shew that its peculiar action may sometimes be thus developed. A female, aged 40, of nervous temperament, was engaged for some time rolling in her hands a paste containing powdered nux vomica. When the operation was completed, she washed her hands carefully; but after a while she perceived a sensation of great heat in her fingers, which constantly increased. At night spasms came on so violently as to prevent sleep; and next morning the fingers exhibited vesicles filled with pus. She was certain that no wounds or pricks had previously existed; but to determine accurately if these effects could be attributed to the powder, some of the paste was applied to two other persons,—of these, one felt nothing, but the other had violent pain and spasms for twenty-four hours.[4]

Camphor, in the solid form, has been little used externally, but Chrestien has spoken very favourably of an application

[1] Vide British and Foreign Review, April 1836, p. 261, &c.

[2] *Sigmond.* Lancet, Feb. 25, 1837.

[3] *Stedman.* Ed. Med. Essays, ii. 41. 1771.

[4] Arch. Gen. xviii. 443; extracted from the Jour. de Chim. Med. Oct. 1828.

denominated the "plaster of Rustaing," the principal ingredients of which are camphor, myrrh, turpentine, and opoponax, in the treatment of "tumeurs laiteuses."[1] And Dr. Burns states that in cases of flooding, the violent retching may be relieved by a camphorated plaster to the region of the stomach.[2]

M. Aumont has related two cases, in which symptoms of narcotism were produced by powdered *Iris* used to the hair.[3]

Dr. Turnbull has spoken in the most enthusiastic terms of the use of an ointment containing *Veratria*, in the proportion of ℈i to ʒi of axunge, in various, and even the most opposite, diseases. Applied in this way to the extent of gr.vi or viii per diem, for several weeks, or even months together, no symptoms of the violent irritation which its internal administration causes will be found to ensue; "for although the constitution has evidently, during the greater part of the time, been under the influence of veratria, so far from acting in that manner, it has been observed to calm irritation and remove pain. The appetite increases,—the bowels are unaffected, or constipated. In dropsical diseases it exerts a most beneficial diuretic effect, but has no such influence on the kidneys in other cases. No irritation of the skin is produced, and sometimes the operator has also been cured by imbibition from the hand."[4]

In conclusion, I shall only add, that according to Dr. Thomson, an *Assafœtida* plaster will act as an antispasmodic;[5] and that Bradner Stuart, while breathing through a tube the air of another room, applied a plaster of *Garlic* to his armpits and thighs, and found that in about half an hour his breath had acquired the peculiar odour, and that in two hours the same was perceptible in his urine.[6]

[1] *Chrestien.* L. c. pp. 58—61.
[2] *Burns.* Principles of Midwifery, 8th edit. Lond. 1832, p. 323.
[3] Vide *Richard.* Arch. Gen. xvii. 285.
[4] Ed. Med. and Surg. Journal, xlii. 155.
[5] *Thomson.* Mat. Med. i. 617.
[6] *Westrumb.* Jour. Complem. xxx. 367.

It results, then, from all the facts here stated, that different solids, when simply applied to the skin, find their way into the interior, but with very various degrees of rapidity; and that their introduction may be accelerated and rendered more certain, by the employment of frictions at the same time.

SECTION II.

Absorption of Liquids.

In considering the effects of various liquid agents applied to the surface, I shall follow the same order as with the solids. First, then,

Of Mercury.—The most important observations which we have to notice under this head are some experiments performed by Séguin.[1] They were as follow:—Having dissolved corr. subl. ʒiii in aquæ ℔xvi, at various temperatures, the legs of several venereal patients were immersed in this mixture as far as the calves, during one or two hours. In thirteen experiments thus performed, at a temperature of 10° R., when the patient's legs were quite healthy and uninjured, no marks of absorption could be detected. But in the fourteenth trial, when the bath was at 18° R., and the patient had a cutaneous eruption, salivation was speedily established. The result of this last observation rendered it uncertain whether the absorption should be attributed to the state of the epidermis, or the increased temperature of the bath, and accordingly five other patients were chosen, of whom two had

[1] *Séguin.* Ann. de Chimie, xc. 198, et seq.; et xcii. 33, et seq.; or *Fourcroy*, La Médecine éclairée, iii. 232, which contains an excellent abstract of both papers.

eruptions, while the other three had perfectly sound legs. The baths of the two first were at 10° R., and the absorption was most sensible; the three last employed baths at 18° R., and the absorption was much less rapid and marked. In some further experiments instituted upon himself, a like result was obtained, viz., a slight absorption of the salt at 18° R., but none whatever at 10° R.

To these experiments, performed as they were with every attention to accuracy, and by one whose skill and candour were undoubted, much weight has been deservedly attached; and if the question of Cutaneous Absorption depended upon the settling of this point alone, its opponents would have better grounds for their opinions than they can at present claim; at least they might, with something like confidence, assert that the influence of this function must be very slight. It is to be observed, however, that the experiments themselves do not warrant its total rejection, since in some of them, even when the cuticle was entire, absorption did take place to a certain extent; and they are, moreover, notwithstanding every precaution, liable to this objection, that, as we all know, some individuals are much less susceptible of mercurial action than others, requiring both a longer time and a greater quantity of the medicine, to produce any appreciable effect; and such may by possibility have been the case in the instances before us. The experiments of which Séguin was himself the subject are not indeed liable to the same fallacy, since the quantity of salt remaining in the bath after immersion, was determined by the quantity of alkali required to precipitate it; and in these absorption was manifest, when the temperature was sufficiently elevated.

But we have besides this the evidence of other authorities, in proof of the efficacy of this salt in the treatment of disease. Thus Auger Ferrerius found salivation produced by the above method.[1] Wedekind has borne testimony to its good effects

[1] *Young.* Thesis, 14.

in various complaints.[1] Verducci has employed it with the greatest success in syphilis. One very remarkable case is related by him, of a patient who had a tumour of syphilitic origin, at the sternal end of the fourth rib, eruptions, and pains of the bones. (The feet and legs were, I presume, unaffected by the eruption, since the author is particularly careful in warning against the employment of this method when there is any breach of the surface.) After the pediluvia had been tried for ten days, the pustules had almost disappeared, the tumour was very small, and the pains entirely gone. On the thirteenth day the patient was cured.[2] Dr. Anderson relates a case of severe salivation, produced by a liniment containing corr. subl. applied to the sound skin.[3] Macquart has spoken favourably of baths of the same substance for the cure of syphilis;[4] and Dr. Guerard has shewn that ptyalism may be thus induced, the proportions being 1 oz. of corr. subl. to 48 oz. of water, and that the effect is commonly developed after the third bath, with an interval of three days between them.[5] The following experiment of Bonfils appears also worthy of notice. He placed upon the abdomen of a syphilitic patient, lying upon his back, a number of drops of a saturated solution of corr. subl. under watch-glasses, and confined by a bandage. In a very short time the water had disappeared, and the surface of the skin, carefully examined by the microscope, shewed no traces of the salt, which it must have done had evaporation taken place.[6] And, lastly, a case lately occurred to Mr. Syme, where a solution of the nitrate of mercury was rubbed by mistake upon the hip and thigh. Intense pain immediately followed, the urine was suppressed

[1] *Wedekind.* Arch. Gen. i. 280, and xxii.

[2] Arch. Gen. xvii. 281; extracted from Osservat. Med. di Napoli, Jan. 1818.

[3] *Anderson.* Ed. Med. and Surg. Jour. vii. 437.

[4] Vide Dict. des Sc. Med.—"Bain."

[5] *Christison.* L. c. p. 392.

[6] Vide *Collard.* Arch. Gen. xi.

for five days, and on the third salivation commenced, became very profuse, and was followed by exfoliation of the lower jaw; but the patient recovered.[1]

Of Iodine.—The grounds upon which I rest my belief of the power which the skin possesses to absorb this remedy, are, its well known efficacy as a deobstruent, when applied to indolent tumours in the form of tincture, an efficacy which, according to Mr. Thorn,[2] is much diminished by its incorporation with lard, or any oily menstruum; and the results of the following experiments, which unequivocally shew how readily it may be detected in the secretions, when simply applied to the surface.

Experiment XXIV.—At 8 A.M. I immersed my arm in a tepid solution of hydriod. pot. ʒv for one hour. My urine at 6 P.M., when tested with starch and sulphuric acid, gave faint traces of the presence of iodine.

Experiment XXV.—At 8 A.M. I immersed my arm during 40 minutes in a tepid solution of hydriod. pot. ʒi. Distinct traces of iodine were observed in my urine, when tested as before.

Experiment XXVI.—Repeated the same experiment for 50 minutes, with a like result.

Experiment XXVII.—Repeated the same for one hour, and the traces of iodine were still more distinct. But in all these experiments the complex nature of the fluid, by concealing in a great measure the characteristic blue colour of the precipitate, rendered the results less conclusive than they might otherwise have been, and I accordingly performed the following experiment:—

[1] *Syme.* Ed. Med. and Surg. Jour. xliv. 26.

[2] *Thorn.* On Sexual Diseases,—reviewed in Lond. Med. Chir. Rev. xvi. 417. 1831.

Experiment XXVIII.—I immersed my arm in the same solution, during periods of different lengths, for four successive days, the whole time of immersion being five hours. My urine was then carefully tested, according to the method recommended by Dr. Christison. A stream of sulphur. hyd. gas was passed through it, in order to convert any free iodine into hydriodic acid. The excess of gas was then expelled by boiling, the fluid supersaturated with potass, filtered, and evaporated to dryness. The residue was then charred in a covered crucible, pulverised, and exhausted with water. The filtered fluid was again evaporated to dryness, and exhausted with alcohol. On evaporating this alcoholic solution to dryness, and dissolving the residue in water, a solution was obtained, which gave a *very deep* blue colour with starch and sulphuric acid. This colour, when diluted, was destroyed by boiling, and partially returned on cooling, but was more distinctly restored by a drop of sulphuric acid.

The result of this experiment I consider perfectly conclusive. It was exposed to no fallacy. My arm was totally free from any wounds, scratches, or abrasions; no friction was employed; the strength of the liquid was not sufficient to cause any local action, and being not in the slightest degree volatile, the lungs could not be considered as the organs by which absorption had taken place. Add to this, the unequivocal proofs afforded by chemical analysis, and more satisfactory evidence cannot, I think, be either required or obtained.

Of Antimony.—It has been shewn by Lebküchner, that tartar emetic rubbed upon the skin of a cat, may be detected in the adipose texture.[1] Mr. Sherwen has performed some experiments upon himself and others, tending to prove that constitutional effects may follow the rubbing of a solution containing this substance upon the palms of the hands.[2] His results have been confirmed by Mr. Hutchinson, but

[1] *Lebküchner.* Arch. Gen. vii. 424, et seq.

[2] *Sherwen.* Mem. of Lond. Med. Soc. ii. 387.

the same experiments completely failed in the hands of M. Savary and Mr. Gaitskell.[1] Chrestien states that M. Fages has cured a case of rheumatism by frictions with a solution of tart. ant. and opium. He has also applied the same application to some venereal excrescences, until nausea, vomiting, and purging caused him to cease; and in thirty days he cured an inveterate gleet by this means, the secretion of urine being at the same time much augmented. But no effects were produced in other cases, and M. Chrestien is not inclined to place much reliance upon the plan.[2] My own experiments tend to confirm, to a certain extent, those of Mr. Sherwen. They are as follow:—

Experiment XXIX.—At 12 P.M. I rubbed into my hands tart. ant. gr.v, dissolved in ʒi of water. Slight tingling was observed at the time, but this may have been owing to the friction. Pulse 64. At 4½ A.M. skin, especially of the hands and feet, hot and dry; mouth parched, and with a disagreeable taste; pulse 64, but fuller and stronger. About one hour afterwards these uneasy sensations gave place to a gentle perspiration, which lasted until I rose. The discharge of urine was copious, but there was no action on the bowels. I examined my urine with sulph. hyd., but could detect no traces of antimony.

Experiment XXX.—I requested a friend to rub into his hands tart. ant. gr.vi, dissolved in ʒi of water. He slept soundly all night, and perceived no physiological effects.

Experiment XXXI.—At 11¼ P.M. I rubbed into my hands tart. ant. gr.x, in ʒi of water, the solution not being complete. Pulse 66. At 4½ A.M., pulse 60, hands and feet hot, the rest of the body bedewed with perspiration. No other effects

[1] *Christison*, p. 452.

[2] *Chrestien*. L. c. note, pp. 10–12.

were observed, and my urine, when tested with hydrosulph. ammon., gave no indications of the presence of antimony. Several small crystals could be seen on my hands next morning, when examined by the direct rays of the sun.

Experiment XXXII.—At 1 A.M. I rubbed into my hands tart ant. gr.x, in about ʒi of water, the solution being quite perfect, but several drops were spilled. Pulse 64. At 8 A.M. my pulse was 60. I experienced in this instance an unusual degree of languor and debility, which continued for some hours, and was accompanied by frequent, though slight attacks of nausea, but my appetite was not impaired. It is necessary to observe, that at the time when these observations were made, I was in perfect health. They would seem to justify the conclusion, that a slight degree of constitutional effect may be produced in this way, but that its success is very uncertain, and not sufficient to hold out any prospect of being useful in the treatment of disease. It will be remarked, moreover, that no local action was excited in any of these instances.

Of Arsenic.—Upon this head I have very little to say. Mr. Sherwen has produced diuresis, by rubbing into his hands a small quantity in solution.[1] But his observations are unsatisfactory; and Lebküchner, after rubbing animals with this substance, could never detect it, either on the inner surface of the skin, or in the fat.[2]

Of Lead.—The evidence in favour of the absorbability of lead is far more satisfactory. Westrumb quotes Schœpf in proof of the production of lead colic and palsy, from its continued external application.[3] Dr. Wall has seen colic induced in one case, by the use of saturnine lotions, for the cure of a pustular disease; and in another, by immersion of the

[1] *Sherwen.* Loc. cit.

[2] *Lebküchner.* Loc. cit.

[3] *Westrumb.* Jour. Comp. xxx. 361.

legs twice a-day for ten days, in a bath of the solution of the acetate.[1] Dr. Mackintosh has met with an instance of the true lead colic, produced by the same application, used to suppress a fœtid perspiration of the feet.[2] Lebküchner rubbed a rabbit with a solution of ʒfs of acet. plumb. in ℥fs of water;—the action of the heart soon became enfeebled, the posterior extremities were paralysed, and the muscles of the abdomen contracted; convulsions followed, and death ensued in twenty-four hours. The internal surface of the skin, and the cellular membrane became blackened by sulph. hyd., but no traces of lead could be detected in either the muscles, fat, or blood.[3] A repetition of the experiment gave similar results. And the following experiments of MM. Seiler and Ficinus leave no room for doubt. The leg of a mare was steeped with an alkaline solution of ox. plumb. for seven hours; the hair at the part was by this time destroyed, the skin inflamed, and blood drawn from the veins gave distinct indications of the metal. The leg of a horse was moistened in like manner, and the same results ensued. The feet of a bitch were steeped in a similar solution for three hours; the animal was then killed, and lead was detected in the blood of the vena cava, and of the vena portarum, in the chyle, and the bile.[4]

Of Purgatives.—Haller states that purging may be caused by the oil of tobacco smeared on the abdomen.[5] Westrumb declares that the same effects are produced by frictions with ol. ricini, and that pediluvia of the white or black hellebore are also purgative.[6] Dr. Wight asserts that catharties, mixed with volatile liniment, and rubbed along the spine three or four times a-day, will keep the bowels open, when a regular course of laxatives has failed.[7] In a London journal

[1] *Christison.* L. c. 517.
[2] *Mackintosh.* Practice of Physic, i. 250.
[3] *Lebküchner.* Loc. cit.
[4] *Seiler and Ficinus.* Jour. Compl. xix. 127.
[5] *Haller.* L. c. 87.
[6] *Westrumb.* L. c. 362–4.
[7] *Wight.* Lond. Med. Repos. xx. 148.

is detailed a case, where the sister and nurse of a ward in Guy's Hospital were employed in rubbing croton oil on the abdomen of a patient labouring under obstinate constipation. In three hours the sister was smartly purged, and experienced peculiar sensations throughout her body, with a nauseous taste. The nurse was also purged, but more slightly. No effect was produced on the patient, a phenomenon easily explained after death, when a mechanical obstruction was discovered.[1] Dr. Traill, in his clinical lectures, states that he has also noticed a similar effect. Dr. Hunter has given the case of some workmen, who were employed to clean out a mineral water well, and for this purpose had thrown off their shoes and stockings. They were all violently purged,—an effect which can scarcely be attributed to the cold, since they had been long accustomed to such employments.[2] My own experiments are also decidedly in favour of this opinion.

Experiment XXXIII.—Having ascertained, by previous experiment upon another person, that gr.x of rhubarb taken internally could be readily detected in the urine, by the red colour and precipitate caused with aq. potassæ, I soaked a large piece of lint in a strong infusion, and applied it to my abdomen at night, covering it with oiled silk, and retaining the whole in its place by a bandage. Next morning my urine was of a natural colour, and gave no indications of rhubarb, either by the smell, or when tested with aq. potass.; but my bowels were slightly affected. Upon referring to Dr. Thomson's Materia Medica, I find that he has noticed the same occurrence, and explains it upon the principle that the substance is not absorbed, but merely acts upon the cutaneous nerves, and by sympathy upon those of the alimentary canal. But how could such an impression be made, if it did not pass through the cuticle? And if it did surmount this obstacle,

[1] Lond. Med. and Surg. Journal, ii. 1828.

[2] Vide *Clare* on Lues, p. 43, 4.

why was it not absorbed? A much easier explanation is to be found in the supposition, that the active parts are alone taken into the system, while the colouring matter is rejected. It is singular, however, that in some of Westrumb's experiments, made by steeping various parts of the body in strong decoctions of rhubarb, the presence of this substance was detected, both in the blood, the lymph, and the urine.[1]

Experiment XXXIV.—I repeated the last experiment, with similar results, but more decided action on the bowels.

Experiment XXXV.—A repetition of the same gave precisely analogous results. I do not, however, find potass by any means a *delicate* test for rhubarb, since two drops of a very strong infusion could not be detected by it, when mixed with ℥i of urine.

Experiment XXXVI.—I performed a similar experiment with *Jalap*. My bowels were more freely opened, and I experienced some griping, and considerable uneasiness, for several hours during the morning. The lint, however, slipped from under the bandage in the night, and much of the fluid was spilled. My urine, when first voided, had a decided odour of jalap.

Experiment XXXVII.—Applied to my abdomen a piece of lint saturated with tinct. jalap ℥i, and covered with oiled silk, which, however, did not prevent pretty rapid evaporation. My bowels were similarly affected, but the urine had no particular odour.

Experiment XXXVIII.—Applied to my groin a piece of lint soaked in a strong alcoholic solution of *Gamboge*, covering

[1] *Westrumb.* Arch. Gen. xxi. 113, et seq.

it as before. Evaporation went on rapidly, and I perceived no physiological effects.

Experiment XXXIX.—Applied to my abdomen a cloth saturated with the same solution of gamboge. Bowels slightly affected.

Experiment XL.—Applied to my abdomen the same solution, with the addition of some water. This prevented the so rapid evaporation, and consequently my bowels were very freely acted upon.

Of Diuretics.—According to Westrumb, squill and digitalis rubbed in solution upon the lower belly, produce copious urination.[1] Chrestien has related several cases of dropsy cured by frictions with the tinct. digit., and remarks that he has never observed any diminution of the pulse, or "orgasme hemorroïdal," from its external application.[2] Alexander steeped a rabbit in a solution of nitre at 110° F., for fifteen minutes, care being taken that the animal did not drink any of the liquid. It was then removed, and immersed again for half an hour, at the end of eighteen hours. In two hours more it was killed, and a paper dipped in the serum of the blood, gave indications of the presence of nitre, by crepitating when burned.[3] He immersed his feet in a solution of the same salt for fifteen minutes,—a copious flow of urine took place in ten minutes afterwards, and a paper dipped in this deflagrated. Schreger has performed a somewhat analogous experiment. An iron spring was placed round the leg of a dog, so as to compress the artery without the vein, and this was opened. The leg was steeped in milk containing nitre, for the space of a quarter of an hour; by this time the lymphatics were found full of milk, and a paper soaked in this

[1] *Westrumb.* L. c. 362.

[2] *Chrestien.* L. c. 207, and note.

[3] *Alexander.* Exper. Essays, 19—31.

deflagrated. The spring was then removed, and blood drawn from the vein, but neither milk nor nitre could be detected in it.[1]

Under this head we may also notice the results of a very important set of experiments, viz., those performed with turpentine. Lebküchner rubbed the skin of a rabbit for twelve minutes with oil of turpentine. After death, both the internal surface of the skin, and the cellular membrane, imparted the smell of turpentine to paper, and the urine had a violet odour. Upon a repetition of the trial, the blood of the inferior cava had the same peculiar odour.[2] Dr. Young, having his head enclosed in an oiled cloth bag, and breathing the external air through a tube, applied turpentine to his arm for half an hour,—it was then carefully washed. He left the room, and walked for one hour in the open air, and by this time his urine had a slight odour of violets, which gradually increased. In a similar experiment with a turpentine ointment, the same results were obtained.[3] Schreger rubbed the arm of a young man, passed through a wall into another room, with turpentine, a tourniquet being applied near the axilla. A vein was then opened, but no smell could be detected in the blood, and his urine evacuated at the same time was destitute of any peculiar odour. The tourniquet was then removed, and both blood and urine speedily acquired the characteristic odour.[4]

Far different, however, were the results of the observations made by Klapp and Rousseau. The former of these immersed his hands and wrists for a quarter of an hour in turpentine, confined over mercury, without perceiving any violet odour in the urine.[5] The latter exposed his body to the vapour of the same substance, while he breathed the air of another room; he even employed frictions with the oil; and in another experiment kept his fingers immersed in the

[1] Vide Jour. Compl. xviii. 327.
[2] *Lebküchner.* L. c.
[3] *Young.* Thesis, &c.
[4] *Schreger.* L. c.
[5] *Klapp.* Inaug. Essay, p. 38.

oil for three hours; but in none of these instances, where pulmonary absorption was carefully avoided, did his urine present any traces of the peculiar odour.[1] Dangerfield and Gordon have, I believe, performed analogous experiments, but as I have been unable to meet with any account of them, I cannot speak with confidence.

Let us for a moment pause to reflect upon these experiments. They have been vaunted as unanswerable, and as at once conclusive of the point at issue. But do they warrant such an opinion? Even supposing that they stood alone,—that we had no evidence to the contrary,—would it be philosophical to deny the existence of cutaneous absorption, because this one particular substance did not permeate the skin? Should we not, if this line of argument were correct, be also entitled to deny that the lacteals absorb, because in the experiments of Flandrin and others, no substance thrown into the bowels, and distinguishable by its odour, colour, or poisonous effects, could be detected in these vessels? And would not such a conclusion be totally at variance with the truth? The cases are quite parallel;—looking to either set of observations alone, and yielding to a spirit of hasty generalization, too common in medical science, physiologists might have been induced to commit the same error in both. With regard to the lacteals, this has been avoided, for the opposite experiments have been carefully noted; but as respects the skin, the observation holds in its full force; and we have thus an admirable example of the great carelessness with which the subject has been hitherto studied. I do not for a moment mean to deny the accuracy of the experiments before us, but I must maintain that, while we have others which give a totally different result, performed with equal care, and by men equally entitled to credit, for I have yet to learn that Klapp and Rousseau are more worthy of confidence than

[1] Vide *Stock's* Account, in Ed. Med. and Surg. Jour. ii. 11; or *Currie*, Med. Rep. i. 340.

Young or Schreger, and when we see that a tourniquet obstructing the flow of blood and other fluids to the heart, prevented likewise the evidence of absorption, it is assuming too much to assert that this substance can only find its way into the system by the lungs. With regard to its absorbability by the skin, in my own person, I am perfectly convinced, and for the following reasons :—

Experiment XLI.—My head being inclosed in an oiled cloth bag, to which a long tube was attached, and passed out of window, so that I was completely excluded from the air of the apartment, and my arm being luted into a large glass jar, at 20 minutes before 3 P.M. ℔i of turpentine was poured upon it, and the jar accurately stoppered. At 26 minutes past 3, that is, 45 minutes from the commencement of the experiment, my arm having become very painful, the jar was removed, and the arm carefully washed with soap and water, while I still continued to breathe through the tube. I then left the apartment, without having once respired in it, and went immediately into the open air, where I remained for some time, the wind blowing freshly. My urine, when first voided at 5 P.M., had a distinct though faint violet odour, which, however, was much increased by 6 P.M., without my having been in any way further exposed to the vapour of turpentine. Unfortunately after this I visited a patient who had been using this remedy, so that all further observations were of no value.

Experiment XLII.—At 3 P.M., the same precautions being taken, my arm was immersed in turpentine for one hour, and then, as before, carefully washed. On account of heavy rain, I could not leave the house, but to avoid all fallacy, I sat for some time at an open window. The urine first voided about one hour afterwards, had a faint violet odour ; this gradually increased, and by 10 P.M. it was most distinct, both to myself and others who examined it.

Experiment XLIII.—I immersed my arm in turpentine for one hour, with precisely similar results, the violet odour being most evident for several hours.

Experiment XLIV.—Repeated the same experiment, with equally satisfactory results.

It may be well to mention here, that Klapp and Rousseau tried infusions of garlic and asparagus in a similar manner, but could not detect any change in the urine. These substances, however, are not very eligible, since, when taken internally in small quantities, they produce but little effect upon the urine. We proceed therefore with the examination of other medicinal agents.

Of Tonics.—Dr. Alexander has related a case of ague, which he cured by the use of pediluvia of the decoct. cinchon. The disease returned, and was again removed by the same means.[1] Chrestien has recorded seventeen cases of the same disease, in which he employed the tinct. quin., either alone, or combined with the tinct. rhæi, with the greatest success; and he states that many medical men in his neighbourhood had informed him that they found the external application more useful than the internal.[2] M. Rullier has found this plan exceedingly beneficial in cases of debility, complicated with local excitement of the digestive organs, and where consequently the internal administration of quinine would have been injurious.[3] Such evidence as this can hardly, I think, be set aside by an experiment of Séguin's, who, while in perfect health, bathed in a solution of quinine, without experiencing any effects,[4] since the same might have resulted from its internal administration.

Of Narcotics.—Every one is acquainted with the relief which is frequently obtained from the external use of opium,

[1] *Alexander.* Exper. Essays, 39, et seq.
[2] *Chrestien.* L. c. 247.
[3] *Rullier.* Dict. des Sc. Med.—"Inhalation."
[4] *Séguin.* Ann. de Chimie, xc. 197.

especially in superficial inflammations and painful swellings, but we have also reason to believe that internal disorders may receive much benefit from the same means. Chrestien has given us the histories of a great number of patients labouring under various disorders, who were very much relieved, if not entirely cured, by frictions with his "teinture antispasmodique," which consisted of crude opium dissolved in brandy; and he informs us that the same application had met with equal success in the hands of others.[1] Dr. Bow has found opiate liniments so useful in the treatment of bronchitis and croup, that he greatly prefers it to blood-letting, and the other ordinary remedies;[2] and Dr. Burns states that the vomiting which generally accompanies flooding may be restrained by applying a cloth dipped in laudanum and spt. vin. camph. to the whole epigastric region.[3] According to Chrestien, one of the great advantages which this method of employing opium possesses over every other, is, that the doses may be greatly increased, without fear of producing narcotism; but Lorry has detailed the account of a woman who was thrown into furious delirium, spasms, and convulsions, by the external use of this drug;[4] and the following case will shew that death may even ensue, under particular circumstances. A soldier, aged 32, was attacked with phlegmonous erysipelas of the right leg, on account of which a linseed poultice, moistened with gtt.xv of laudanum, was ordered to be applied. Next morning he was found in a state of deep sopor, with the face pale, the eyelids tremulous and half open, the pupils contracted, the lips distorted, the muscles of the face affected with spasms, and those of the limbs with convulsions. The surgeon having remarked a strong odour of opium, as well as a yellow coloration of the bandages, had them removed, and found all the dressings soaked with laudanum, which was

[1] *Chrestien.* L. c. pp. 63—168.

[2] *Bow.* Lancet, ii. 1834–5, p. 721; and ditto, March 18, 1837.

[3] *Burns.* Midwifery, 323.

[4] *Thomson.* Mat. Med. i. 506.

flowing copiously from the poultice, and it was discovered that about ℥i of the tincture had been used. The patient died, and on dissection a strong opiate odour was exhaled from all parts, but no trace of the poison could be detected in the blood.[1]

Lastly, the experiments of Dr. Monro are conclusive as to the action of this poison through the skin of frogs.[2] In these experiments, which were frequently repeated, and performed with the greatest care, the application of an aqueous solution of opium to the cutaneous surface was followed by insensibility, convulsions, and death; and as a proof that these effects were not owing to pulmonary absorption, as some have alleged, the same application was found to be perfectly innocent in two trials, when the aorta, vena cava, and lymphatics were tied at the upper part of the pelvis.[3] The same fact is perhaps still more decidedly shewn by the following

Experiment XLV.—I confined a frog over a vessel containing a strong aqueous solution of opium, and covered by a screen of wire gauze, to prevent any contact of the fluid, for twelve hours, without its being in the slightest degree affected. Lint soaked in the solution was then applied to its back. When examined twelve hours afterwards it was found convulsed, and much less sensible to stimuli, and in some hours after it died.

It would appear, however, from the next experiment, that some cold-blooded animals are less rapidly affected.

Experiment XLVI.—I immersed a water newt in the same solution, its head projecting through a screen of oiled silk, so as to prevent any from entering the throat or lungs. It was still alive and sensible, though less so than

[1] Ed. Med. and Surg. Jour. xxix. 450; extracted from Jour. de Chimie Med. Avril, 1827.

[2] *Monro.* Physical and Literary Essays, iii. 297, et seq.

[3] P. 316.

natural, at the end of thirty-four hours. It died within the forty-eight hours.

Of Belladonna.—Mr. Blaekett has found a strong tineture of belladonna, diluted with water, extremely useful in several eases of superfieial inflammation.[1] Mr. Robb has related four eases of neuralgia, in whieh the greatest relief was obtained from smearing the skin, along the course of the nerve, with a watery solution of the extract. When the applieation was discontinued, the pain returned, but was again removed by a recurrence to the remedy.[2] Mr. Holbrook has eured retention of urine by fomentations of belladonna.[3] M. Couty de la Pommerais has related a ease of poisoning, by ʒii of the extract in a liniment, and gr.x in an enema.[4] Leclercq has cured a ease of neuralgia by lotions with an aqueous solution of the extract, partial blindness being for a time produced;[5] and the same means proved equally successful in the hands of M. Audibert.[6]

Of Tobacco.—The production of vomiting from the application of tobaeco in deeoetion, to the epigastrium, is well known, and it would appear that the same may result when other situations are ehosen, as for instanee the sealp.[7] The powerful sedative effects thus produeed have been taken advantage of by many practitioners, in the treatment of various affeetions; and the method has this peeuliar exeellenee, that the doses can be more aeeurately regulated, and the effeets taking plaee less rapidly, are more under eontroul than when it is administered internally. Thus, Dr. Graves of Dublin has reeommended the deeoetion to be applied to the abdomen, in eases of painter's colie, until its eonstitutional effects are

[1] *Blackett.* Lond. Med. Repository, xix. 451.
[2] *Robb.* Trans. of Associated Apothecaries, i. for 1823.
[3] *Holbrook.* Lond. Med. Repository, 1823, p. 289.
[4] *Couty de la Pommerais.* Arch. Gen. xvii. 107.
[5] *Leclercq.* Ibid. 113. [6] *Audibert.* Ibid. 602.
[7] *Haller.* L. c. 86, 7; and *Thomson,* Mat. Med. i. 437.

produced.[1] Dr. Vanderburgh of New York has employed the same means with great success, to allay the irritative cough arising from various diseases of the lungs, to moderate spasm, and to favour the reduction of herniæ.[2] Dr. Vetch finds it very useful in gout, and rheumatic inflammation attacking synovial membranes, also in erysipelas, ophthalmia, and swelled testes.[3] An anonymous correspondent of the Lancet speaks favourably of tobacco pediluvia, in the treatment of gout.[4] A friend of mine, a gentleman in extensive practice in England, informs me that he is acquainted with no method so successful in the treatment of glandular inflammations, as the application of a tobacco cataplasm. Even that most intractable of diseases, traumatic tetanus, appears in some instances to have yielded to the power of this remedy, Dr. Anderson of Trinidad having successfully treated two cases with tobacco baths.[5]

But although the external application of this drug is less dangerous than its internal use, the following observations shew that it may yet prove fatal, if carried too far. Sir A. Cooper has related a case, in which death was produced from the application of tobacco juice to the head, as a remedy for tinea.[6] Mr. Colville has killed dogs in this manner;[7] and in my own hands the same application destroyed a rabbit.

Experiment XLVII.—At 11¼ A.M., I inclosed a strong healthy rabbit in a bladder, to which a stop-cock was attached, and which was carefully fastened round the animal's neck. A strong decoction of 2 oz. of tobacco was then introduced, but the quantity not being nearly suffi-

[1] Vide Arch. Gen. xvii. 119.
[2] *Vanderburgh.* Lond. Med. and Surg. Jour. ii. 478.
[3] *Vetch.* Med. Chir. Review. xvi. 488. (1831.)
[4] Lancet, March 18, 1837.
[5] *Anderson.* Ed. Med. Chir. Trans. i. 184.
[6] *Cooper.* Lectures on Surgery, 8th edit. 1835, p. 434.
[7] *Colville.* Ed. Med. and Surg. Jour. xvi. 472.

cient to cover its whole body, I endeavoured, by shaking it about, to wet as much of the surfaee as possible. At a quarter to 12, general trembling was observed, whieh gradually inereased, and beeame accompanied with a tendeney to fall forwards. At 2+25′ P.M. the symptoms were mueh aggravated; the animal appeared as if intoxieated, shaking violently, and seareely able to stand a moment. Death took plaee at 5 P.M., having been preceded by convulsions. Upon disseetion the lungs were found mueh congested, and the mesentery greatly injeeted. The eontents of the eranium presented no unnatural appearanee.

Of Nux Vomica.—This aetive poison does not appear to exeite any peeuliar aetion, when simply applied to the sound skin, though, as we have already seen, when friction is at the same time employed, serious results may follow. Delile applied strychnia in this way, without produeing any eonvulsions,[1] and in my own trials I was equally unsuceessful.

Experiment XLVIII.—I dropped into the axillæ of a pigeon about 80 gr. of a strong aleoholic extract of nux vomica, but no appreeiable effects were produeed, the animal appearing quite unaffeeted.

Experiment XLIX.—I repeated the same experiment, with preeisely the same results. It must be aeknowledged, however, that these experiments are not conelusive. M. Despartes has found that nux vomiea produees very little effect upon poultry,[2] and probably pigeons may share the same immunity; at least they do not seem to be very susceptible of its influence, for in a subsequent trial, I inserted ℈i of a somewhat weaker extraet into a wound of the baek of this same pigeon, and in three quarters of an hour not the slightest effect could be observed. It was then killed for another purpose.

[1] *Thomson*, i. 252.

[2] Ibid. i. 251.

Of Hydrocyanic Acid.—This very virulent poison has been little used externally, but Drs. Buttigny and Lombard of Geneva have successfully employed the cyanide of potassium, both in the state of solution and in the form of ointment, in the treatment of neuralgia. Dr. L. thinks the solution acts more promptly, but both are efficacious. They have found this method useful also in chronic rheumatism.[1] Dr. Bloglia dal Persico has related ten cases of neuralgia which were cured by the application of cherry-laurel water, all having been previously treated in the usual way, without relief.[2] Dr. Thomson has found the bitter almond emulsion a good remedy in impetigo, and states that Celsus also employed it.[3] He once used the oil of the same plant, largely diluted with spermaceti ointment, but the effects were such as to prevent him from employing it again.[4] We cannot wonder at this result, since Emmert has found the pure oil rapidly fatal, when dropped on the sound skin of the back of a rabbit.[5] Dr. Thomson, however, would attribute to it a more purely local action, since he states that when hydrocyanic acid is applied to one limb only of a frog, this member becomes paralysed, whilst the other limbs remain unaffected;[6] but the results of my own observations lead me to rely implicitly upon those of Emmert, and to believe that the greatest care is necessary in the external application of so potent a remedy.

Experiment L.—At 5 minutes past 11 A.M., having cut the hair close on the back of a strong rabbit, but without injuring the skin in the slightest degree, I poured on it about gtt.xx of the essential oil of bitter almonds, and then covered it with oiled silk and a bandage. For some minutes

[1] *Thomson*, i. 431.

[2] Vide Ed. Med. and Surg. Jour. xlii. 483,—from the Annali Univers. di Med. 1832.

[3] *Thomson*, ii. 433.

[4] Ibid. 436.

[5] *Christison*. L. c. 698.

[6] *Thomson*, p. 417.

the animal appeared stupid, but soon recovered its liveliness, and was quite unaffected at 18 minutes past 2 P.M., when an additional quantity was dropped on the axilla. The same drowsiness ensued, and again disappeared, and at 20 minutes before 3 P.M., another quantity was applied, without any more unequivocal results, and the animal was quite lively when I left the room about $3\frac{1}{4}$ P.M. It died, however, during the night, but as the bandages had been removed, a small quantity might have found its way into the mouth, and the experiment was therefore by no means conclusive. I accordingly performed the following.

Experiment LI.—At 5 minutes before 8 A.M., having cut close the hair in the axillæ of a strong rabbit, I dropped on the skin in these places ℥i of the same oil, and as before, covered it with oiled silk and a bandage. The animal soon became very stupid, but gradually recovered, and appeared little affected until half an hour had elapsed, when suddenly, and without any previous warning, the breathing became remarkably accelerated. Violent tetanic convulsions speedily ensued, the respiration became slower, and coma gradually stole on. At 22 minutes to 9, the eye was quite insensible to every stimulus. Respiration continued very laborious, and death took place at 9+5′.

The skin was removed by an incision made along the back; the whole body had a strong smell of the oil, but especially the axillæ, and great venous congestion was found in the internal organs. It may be remarked also, that the former experiment, where the animal was exposed for many hours to the vapour of an equal quantity of the oil, without any particular affection, affords another proof, that death in this instance was not owing to pulmonary absorption. But independently of this, the question was decided by the next experiment, in which this chance of fallacy was totally avoided.

Experiment LII.—At 10 minutes before 11 A.M. I dropped 3fs of the oil into the axillæ of a pigeon, and then introduced it into a jar, closed at the mouth with oiled silk, through which its head alone protruded, and was carefully luted. At 11+5′ the breathing became accelerated. In two minutes more the bird vomited. At 11+12′ respiration was very laborious, and became gradually worse until 11+23′, when a fit of convulsions came on. After this the breathing grew slower and slower, and death took place at 10 minutes past 12, being 1 hour and 20 minutes after the commencement of the experiment.

Of Camphor.—Four cases of retention of urine from various causes, are related by Mr. Latham, in which, after all other means had failed, he effected a cure, by means of a strong camphorated liniment applied to the thighs.[1] Chrestien has found the same means very successful in these, and numerous other affections, and he quotes the testimony of many other practitioners in favour of the plan.[2] Lebküchner rubbed the abdomen of a rabbit for 12 minutes with a camphorated oil ; the animal was then seized with tottering and hurried respiration ; it was killed, and paper placed carefully under the skin, acquired the peculiar odour of camphor, which was also present in the blood of the inferior cava.[3] But Klapp could perceive no change in his urine, after steeping his hands for some time in tincture of camphor.[4]

According to Lebküehner, a solution of *Mur. Barytæ* rubbed on the skin of a rabbit, produced death, preceded by all the symptoms of narcotic poisoning, and the presence of the salt was detected in the blood.[5]

Emmert has found gallic acid in the urine, after the external application of a decoction of false *Angustura* bark.[6]

[1] *Latham.* Med. Commun. ii. 138.
[2] *Chrestien,* p. 15, et seq.
[3] *Lebküchner.* L. c.
[4] *Klapp.* L. c. 41.
[5] *Lebküchner.* L. c.
[6] *Emmert,* Arch. Gen. vii. 582.

Fontana informs us that a person employed in gathering the leaves of the *Rhus Toxicodendron* became affected with a violent inflammation of the face, arms, and neck; and similar effects were produced in his own person by the application of a very small quantity of the juice, and on one occasion by simply touching the leaves.[1]

Sedillot,[2] Martin,[3] and Desparanges,[4] all bear testimony to the great relief afforded by the use of frictions with *Acetic Ether* in rheumatism.

Tic douloureux has been cured by the application of *Tar*.[5]

Baths of *Vinegar* have been used with success in the treatment of scurvy.[6]

Oil of Amber used externally, has antispasmodic powers;[7] and the West Indian practitioners extol *Naphtha* as an external application, in croup and other spasmodic diseases.[8]

Musk has been found by Chrestien, and many others, exceedingly useful in chorea, diarrhœa, dysentery, and passive uterine hæmorrhage.[9] Soemmering declares that pediluvia of this substance cause its peculiar taste to be felt in the mouth;[10] and though Rousseau was unable to detect it in the urine, when pulmonary absorption was avoided, the following experiment of Schreger's shews unequivocally that it can be introduced through the skin. In bleeding a woman from the foot, a lymphatic vessel had been divided, and a quantity of lymph continually flowed from the wound. The lower part of the foot was then immersed in tepid water, mixed with a solution of musk; the lymph was collected in

[1] *Fontana.* On the Venom of the Viper, &c., ii. 183, 4.
[2] *Sedillot.* Rec. Pér. de la Soc. de Med. à Paris, ii. 285.
[3] *Martin.* Rec. Pér. viii. 61.
[4] *Desparanges.* Ibid. 318.
[5] *Colville.* Ed. Med. and Surg. Jour. x. 288.
[6] *Wilkinson.* Med. Mus. ii. 194.
[7] *Thomson.* Mat. Med. i. 614.
[8] Ibid. 620.
[9] *Chrestien,* p. 44, et seq.
[10] *Westrumb.* Jour. Compl. xxx. 365.

a cupping glass, and blood was drawn from a vein on the dorsum of the foot. The result was soon apparent,—the lymph speedily acquired the odour of musk, while none whatever existed in the blood.[1]

Dr. T. Sewall found the urine coloured, after bathing the feet in fusion of *Madder*, and the hands in infusions of madder and rhubarb; and Dr. Mussey has proved that after the body has been bathed in infusion of madder, it may be detected in the urine, by its appropriate alkaline tests.[2]

The effects which follow the continued use of *Nitro-Muriatic* baths, afford abundant evidence of the absorption of chlorine. They are, weakness, occasionally fainting, restlessness, and nervous irritability, a coppery taste in the mouth, increased flow of bile, and perspiration, the urine also acquiring the characteristic property of destroying vegetable colours.[3]

Lastly, Lebküchner having rubbed a rabbit with a solution of *Ferrocyanate of Potass*, detected its presence in the blood, the fæces, the chyle, and water in which the lungs had been washed. Westrumb, after keeping the hands and feet immersed for some time in solutions of the same salt, has detected it in the urine, the serum discharged from a blister, and the blood. A dog, whose skin was in a perfectly natural state, gave similar results, when treated in the same way, the blood and urine containing the salt, while none could be found in the chyle or lymph; but in another dog, whose skin had been previously inflamed by frictions with tinct. lyttæ, it was detected in all the fluids.[4] For my own part I have been less successful, being never able unequivocally to discover it in the urine, as the following experiments will testify:—

[1] *Schreger.* Jour. Compl. xviii. 327.

[2] *Dunglisson.* Human Physiology, ii. 68, 2d edit. Philadelphia, 1836.

[3] Vide *Bell,* Surg. Obs. part iii. 360. *Scott.* Med. Chir. Tr. Lond. viii. part i.; and *Johnson.* On Derangements of the Liver, p. 112. Lond. 1820.

[4] *Westrumb.* Arch. Gen. xxi. 113—117.

Experiment LIII.—I immersed my hand and arm in a glass jar, containing a solution of ferrocyanate of potass ℥ii, in about three pints of warm water, and retained it there during one hour. My urine, when examined immediately afterwards, and at two other successive intervals, by means of permur. ferri, gave but very unsatisfactory indications of its presence; the precipitate, where it adhered to the side of the glass, having a faint blue colour.

Experiment LIV.—I immersed my arm for an hour and a quarter in a tepid solution of ℥ss of the same salt, but no traces of it could be detected in my urine.

Experiment LV.—I immersed my arm in the same solution, during successive periods, for three days, until the whole time amounted to three hours and ten minutes; but my urine, though examined with the greatest care, did not appear to contain any ferrocyanate.

The result of this experiment surprised me exceedingly at the time, but a further examination of the subject has shewn me that it was conducted upon erroneous principles, since all observers are agreed that so far from being a cumulative substance, the rapidity with which it is discharged from the system is so great, that a very short time elapses after its internal administration, before it has almost disappeared. Thus Wetzer, after taking ℥i, could only obtain gr.iv of the blue precipitate from a considerable quantity of urine, collected at various periods. And accordingly, some hours having elapsed, in my experiment, after the last immersion, (which was only for half an hour), before my urine was examined, it could scarcely be expected to succeed. Nevertheless I was equally unfortunate in my next experiment, where this source of error was avoided; and I find that Seiler and Ficinus also failed in detecting it, either in the lymph or blood of a mare, whose foot had been steeped in a solution of the salt.

Experiment LVI.—I immersed each of my arms for two hours, in a solution of ʒi of ferrocyan. pot., but none of the salt could be detected in my urine, though carefully examined immediately after the experiment, and at several other periods, both with the permur. ferri. and sulph. cupri.

Be this, however, as it may, the evidence already adduced is, I trust, too full, complete, and satisfactory, to be even materially weakened, much less set aside, by the unfavourable results of any one experiment. Indeed, it would be unreasonable to suppose that all liquids, without exception, should be uniformly absorbed by the skin.

SECTION III.

Absorption of Gases and Vapours.

In this part of our subject it will be impossible to preserve the same order which we have followed in the two preceding sections. I shall, therefore, in the first place, consider the physiological effects resulting from the application of certain gases to the surface of the common integuments, and then notice briefly the employment of fumigations in the treatment of disease. Under the first head, we meet with a subject of the greatest interest, and no little obscurity; one which has been long and keenly debated among physiologists, and which still remains in a great measure unsettled,—I mean the question of Cutaneous Respiration. In many of the lowest tribes of animals, which are unprovided with any sepa-

rate organs for respiration, this most essential process is evidently carried on by the surfaee of the skin. But as we advance higher in the seale of ereated beings, we find a particular provision for this purpose, in the formation of a set of organs, more or less eomplieated, and suited to the peculiar eonditions under whieh the animal is plaeed. But although in these ereatures the influenee of the skin is undoubtedly mueh limited, we have yet suffieient reason to believe that it is not altogether annihilated; that a eertain degree of ehange is effeeted in the air by this surfaee, analogous to that whieh takes plaee in the lungs or branehiæ. "In inseets," says Spallanzani, "I had reeourse to those whieh breathe at the extremity of the tail, and whieh Reaumur called rat-tailed vermes. By tying their tails tight with a fine silk thread, I deprived them of this benefit, and by this means ascertained that the external surfaee of their bodies destroyed a part of the oxygen of the atmospherical air, although this destruetion was more eonsiderable when the action of the respiratory organs remained unimpaired."[1] He determined, in like manner, that fishes and serpents absorbed oxygen from their skins, though not in any great quantity. But by removing the lungs of oviparous quadrupeds, he found that "the eonsumption of oxygen gas by the lungs is extremely small, in comparison with that whieh is absorbed by the external surfaee of their bodies." Birds and quadrupeds gave a similar result, a greater or less absorption of oxygen taking plaee in all of them.

MM. Provencal and Humboldt found that water in whieh the bodies alone of fishes had been eonfined, afforded the same aeriform products as when they were permitted to breathe by their gills.[2]

Dumeril eut off three-fourths of the head of a triton marmoratus; in the proeess of eieatrization, the apertures of res-

[1] *Spallanzani.* Memoir on Respiration, edited by Senebier. Lond. 1804. p. 118.

[2] *Ellis.* Enquiry on Changes of Air, part ii. 353.

piration and deglutition were completely closed; it nevertheless lived for three months, and then died from accidental neglect.[1] And, lastly, the elaborate and extended researches of Edwards, by confirming the observations of Spallanzani, have completely decided the question with regard to amphibia. Some frogs were strangled, by tying very tightly with packthread a piece of bladder fitted closely to the head, so as to completely exclude the air. They were then inclosed in receivers, and at the end of one or two hours the air contained a sensible quantity of carbonic acid.[2] A frog confined in water which was frequently changed, lived for two months; while others immersed in water from which the air had been expelled by boiling, died in a few hours. Six frogs were strangled by a ligature passed behind the head, the most rigid compression being thus obtained, and placed on wet sand. They lived a considerable time,—one of them for twenty days. The same animals would have died in from one to three days, if they had been placed in ℥vfs of water.[3] As the severity of this operation might, however, have tended to shorten life, another plan was adopted, viz., the entire removal of the lungs. This was easily performed, and the animals did not appear to suffer much. They were then placed upon moist sand, and of three thus treated, two lived thirty-three days, the other forty days.[4] Upon examining the converse proposition, viz., the possibility of life being prolonged by pulmonary respiration alone, it appeared from experiments upon tree frogs, and the rana obstetricans, that in these animals pulmonary respiration is not sufficient to support life, without being accompanied by the atmospheric influence upon the skin;[5] and the same was also observed of lizards.[6]

But it is time that we should now attend to man. The Count de Millet was the first who examined the air given off

[1] *Bell.* Cyclop. of Anat. and Phys.—"Amphibia."
[2] *Edwards.* L. c. 12, et seq.
[3] P. 36.
[4] P. 37.
[5] Pp. 40, 41.
[6] P. 65.

by the surface of the body in the warm bath, and concluded it to be carbonic acid.[1] Cruickshanks detected the same in air in which his foot had been confined.[2] Gattoni inclosed some young persons in leather sacks, which were carefully fastened round the loins, and found that the bulk of the air was diminished, and its constituents altered.[3] Jurine found the air contained in bottles fastened to different parts of the body, to become vitiated, and he concluded that the extent and rapidity of this change is proportioned to the activity of the cutaneous vessels.[4] But Dr. Priestley, on a repetition of the experiment, could not perceive any such change, and he affirms that the carbonic acid obtained by Millet came from the water, not his skin.[5] Abernethy introduced his arm into a jar containing atmospherical air, and inverted over mercury, every precaution being adopted to detach any gaseous matter which might be adhering to the surface. This process being repeated at various times for five hours, the air was then found to be diminished ℥ss, and upon lime water being introduced, ℥i was rapidly absorbed, and the remaining air examined with nitrous gas, contained one-sixth less of oxygen than before the experiment.[6] In another similar experiment, after nine hours' exposure, more than ℥i of carbonic acid was produced, and the oxygen was diminished by one-fourth. After exposure of his hand for five hours to nitrogen, hydrogen, and nitric oxide, carbonic acid was produced; no oxygen could be detected in the remainder, and no diminution of bulk had taken place. Upon performing a similar experiment with carbonic acid, great absorption took place, and much nitrogen was evolved; and on comparing the effect

[1] *Wood.* On the Skin, 38, et seq.

[2] *Cruickshanks.* On Insensible Perspiration, pp. 81, 82, 8vo. Lond. 1795.

[3] Dict. des Sc. Med.—"Peau."

[4] *Ellis.* L. c. 354.

[5] Vide *Wood.* On the Skin, &c.

[6] *Abernethy.* Surgical and Physiological Essays, 8vo. Lond. 1793—7, p. 119, et seq.

of exposure to nitrogen and oxygen, it appeared that in eight hours $\frac{1}{8}$ in. of the former had been absorbed, and one inch of the latter. The whole course of his observations led him to conclude that the absorption of air from the skin is greater than the secretion, and that if the naked body were exposed to fresh currents of the air, the oxygenous parts only would enter the body.

These results have been in some measure confirmed by Dr. C. Mackenzie ;[1] but Fourcroy and Séguin deny that any such action can take place.[2] Klapp has related several experiments, in which no change was produced, in either air or oxygen gas, by exposure of the hand for three hours, and very little absorption with carbonic acid ;[3] and Dr. Gordon was equally unfortunate in his observations.[4]

The very contradictory nature of this evidence renders it extremely difficult to draw any definite conclusions; but when we consider the analogy which may be traced between the skin of man and the integuments of those animals in which the existence of this function is so decided ; when we reflect, that in all physiological inquiries, a *positive* result is of far greater importance than any *negative* ones ; and when we add to this, our knowledge of the facility with which various gases intermix with each other, or penetrate into fluids through the medium of animal membranes, we shall, I think, see reason to believe, that those observers have not been entirely deceived, who have attributed to the skin a share of that process, which, in its full perfection, is carried on at the surface of the pulmonary mucous membrane. That during health its influence must be very slight, the experiments before us certainly appear to indicate ; but the case may be different in disease, and we may perhaps obtain in this way some explanation of a fact very difficult to be understood upon any other supposition, viz., the continuance of life, when the

[1] *Ellis.* L. c. 357.
[2] *Wood.* L. c.
[3] *Klapp.* L. c. 24—6.
[4] *Ellis.* 355, 6.

greater part of the lungs has been rendered utterly incapable of performing its functions; and perhaps the conjecture of Pariset may not be altogether unfounded, that the augmented heat of the hands and feet, and the redness of the countenance in phthisical patients, may be connected with the increased activity of this process.[1]

But besides this, the observations to be now mentioned, shew unequivocally that certain gases, when applied to the cutaneous surface alone, are capable of exerting a deleterious influence. Thus, a patient of Dr. Beddoes experienced languor, coldness, and faintness, from immersing her hands and arms in fermenting yeast.[2] M. le Comte Chaptal affirms that limbs plunged in *carbonic acid* are greatly benumbed; and M. Collard de Martigny, therefore, wishing to determine what would be the effect of this gas upon the whole surface of the body, placed himself entirely under the cloth which covered a deep tub, partly filled with fermenting grapes; and his nostrils being accurately closed, he breathed through a long tube which communicated with the pure external air. "In about five minutes," says he, "I felt a slight weight in the head, and troubled vision; in eight minutes, considerable pain of the temples, and under the orbit, ringing of the ears, and vertigo; in ten minutes, the same symptoms remained, with general weakness, and the heart's action became slightly accelerated; in twelve minutes, all these were aggravated, respiration became slow, a vague and indefinable terror seized my senses; and finally, at the twenty-ninth minute, the weakness and torpor were so pronounced, that the tube by which I breathed fell from me, and I could scarcely leave the spot."[3] In a subsequent experiment, he inclosed a sparrow in a vessel filled with carbonic acid, the head alone projecting

[1] Dict. des Sc. Med.—"Inhalation."

[2] *Beddoes.* On Factitious Airs, iii. 66. Bristol, 1794—6.

[3] *Collard.* De l'Action du Gaz Acide-carbonique sur l'economie animale. Arch. Gen. xiv. 211, 12.

through parchment, stretched across the bottom, and fastened round its neck. In three quarters of an hour respiration became hurried, the eyes fixed, the eye-lids closed, and the body violently agitated; death occurred in an hour and three quarters. Another bird, treated in the same way, died in two hours, and a third in an hour and a half.

The only experiment which I have myself performed with this gas, gave a totally different result, but one which may be perhaps attributed to the dense arrangement of the feathers in pigeons, the animal employed.

Experiment LVII.—At 11 A.M. I introduced a pigeon into a jar, closed at the mouth with oiled silk, through which its head was protruded, and carefully luted. The jar was then filled with carbonic acid, but at 2 P.M., no effect having been produced, I removed the bird.

Hydro-Sulphuric Acid, the gas to which we shall next attend, acts with far greater rapidity. Chaussier inclosed a dog in a bladder of this gas, so arranged that he could easily breathe the air. The animal soon showed signs of suffering, respiration being frequent, the eyes dim, and the ears cold; convulsions ensued in eight minutes, and death took place in ten. This experiment was frequently repeated, always with the same result, and birds treated in a similar manner soon likewise perished. When one leg only of the animal was exposed to the gas, the poisonous effects came on in twenty minutes, and death took place in thirty minutes.[1] Lebküchner found that a rabbit could be killed by this means in ten minutes, and he detected the presence of the gas in the subcutaneous cellular membrane, and in the blood of the inferior cava, by means of polished pieces of lead and silver, and a solution of the acet. plumb. The muscles were found black, and their irritability destroyed.[2] Nysten has also re-

[1] *Chaussier.* Jour. Gen. de Med. par *Sedillot*, xv. 24—36.

[2] *Lebküchner.* Arch. Gen. vii.

peated the same experiments upon various animals, with precisely similar results.[1]

My own observations corroborate those just related, in every respect.

Experiment LVIII.—Having procured a bladder, fitted with a stop cock at one end, and an opening at the other, a young and healthy rabbit was introduced into it as far as its neck, round which the bladder was fastened with strings, as tightly as possible without producing strangulation. The air within the bladder was then pressed out, and replaced with sulph. hyd. At the end of three minutes its breathing became much accelerated. After $11\frac{1}{2}$ minutes the animal fell on its side, and in $1\frac{1}{2}$ minutes more convulsions came on, attended with very difficult respiration. These symptoms progressively increased, and in sixteen minutes from the commencement of the experiment, the animal appeared dead, and was accordingly removed from the bladder, and conveyed to another room. Here, however, it made two or three more convulsive expirations, and then died. The body presented no particular appearances, but a solution of acet. plumb., poured beneath the skin, and into the cavity of the peritonæum, gave distinct traces of the presence of this gas. During the course of the experiment, a small quantity of the gas escaped from the bladder, but not sufficient to account for the effects observed. It may be well to remark that the anus was covered with adhesive plaster.

Experiment LIX.—I repeated the same experiment, taking the additional precaution of luting round the neck with putty. Death took place in eleven minutes, with similar phenomena as in the last. Notwithstanding all my care, some gas still escaped; but that the fatal event was not owing to this cause,

[1] *Nysten.* Recherches de Physiologie et de Chimie Pathologiques, p. 128, 8vo. Paris, 1811.

is clearly manifested by the fact, that another rabbit, placed close to it upon the same table, was scarcely, if at all, affected.

Of Chlorine.—Dr. Beddoes introduced the arm of a negro into a large jar full of this gas, and the back of his fingers lay in some water at the bottom of the vessel, impregnated with the same. He had ulcers from the itch, between his fingers, which gave him so much pain, that in twelve minutes his hand was withdrawn. By this time, however, the back of his fingers had acquired an appearance as if white lead paint had been laid upon them, a change which, however, did not prove permanent.[1] But the most full and satisfactory observations are those for which we are indebted to Dr. Wallace. This gentleman has detailed several cases of obstinate liver complaints, which he has succeeded in curing, by the application of dilute chlorine to the surface of the body, great care being taken that none should enter by the lungs. The effects produced in this manner are, a general sensation of tingling of the skin, with increased sensibility, copious perspiration, a papular rash, great increase of the quantity, and change in the quality of the bile, copious salivation, soreness of the mouth, fauces, and œsophagus, followed by minute superficial ulcerations. The urine loses the power of reddening litmus paper, and acquires the peculiar property of destroying vegetable colours to a greater or less extent.[2]

Buchner, when preparing *Cyanogen* gas, found on one occasion, that the finger which was exposed to the bubbles as they escaped, became suddenly benumbed, and that this effect was attended with a feeling of pressure and contraction in the joints of the thumb and elbow ;[3] and Robiquet once observed a similar effect from the vapour of hydrocyanic acid.[4]

It would even appear that the emanations from plants may, under some circumstances, act through the skin, since Viridet and Scopoli affirm that weakness is produced by twigs

[1] *Beddoes.* L. c. i. 32.

[2] *Wallace.* Researches on Chlorine, 99—112.

[3] *Christison,* 756.

[4] Ibid. 698.

of aconite placed in the bosom; and according to Ettmuller, the roots of white hellebore applied to the belly cause vomiting;[1] since Miller informs us, that the roots of hyosciamus hung round the neck, are of so much use to children, that they are manufactured into beads for this purpose, and sold under the name of anodyne necklaces;[2] and since Richard has related the case of a Dr. Dickson of Edinburgh, who, from carrying some flowers of the momord. elaterium in his hat, perceived in about half an hour a severe headache, with a feeling of constriction about the temples and forehead, almost immediately succeeded by pain in the epigastrium, and colic. Soon afterwards he had several watery evacuations, and in three hours he vomited all the contents of his stomach, mixed with bile. These symptoms, which had commenced at 5 P.M., continued until next day, were accompanied by fever, and left great weakness.[3]

We turn, however, to examine the influence of substances applied to the body in the form of vapour; and, first,

Of Mercury.—Fumigations with this mineral have been much employed in the treatment of syphilis. Lalonette speaks very favourably of this practice.[4] Rapou relates many cures effected by this means, and states his belief that mercury under this form is more active than under any other.[5] One case recorded in his article "Vapeurs," in the Dict. des Sc. Med., is worthy of especial notice. The patient was afflicted with what he terms "pharyngo-laryngeal phthisis," of a syphilitic origin, which had already proceeded to such an extent, that deglutition was impossible. In this state mercurial vapour baths were administered. After the eighth, the pains began to disappear, and the power of deglu-

[1] *Haller*, v. 87.

[2] *Sigmond.* Lancet, February 25, 1837.

[3] *Rickard.* Arch. Gen. xvii. 285.

[4] *Kellie.* Ed. Med. and Surg. Jour. i. 170.

[5] *Rapou.* Traité de la Méthode Fumigatoire. Reviewed in Arch. Gen. vii. 319; and Dict. des Sc. Med.—"Vapeurs," by the same.

tition returned, and by the tenth a perfect cure was established. Dr. Gibson informs us, that it is a method which has been long employed by the native practitioners in India; and he relates several cases of rheumatism and syphilis, successfully treated by himself, salivation being produced in all.[1] Mr. Abernethy has also borne ample testimony to its efficacy. His plan is to place the patient in a vapour bath, in a complete suit of under garments, with a cloth covering the bath. ʒii of the grey or protox. of mercury are then put on a hot iron, placed within the bath. The patient remains there for fifteen or twenty minutes, in which time the whole body becomes covered with a white powder. He is then put to bed in the same clothes, and lies in them till next morning, when he enters the tepid bath. This Mr. Abernethy regards as the most gentlemanly way of curing syphilis, and states that he has seen it produce salivation in 48 hours.[2]

It must be confessed, however, that in the hands of other practitioners this method has not been equally successful. The late Mr. J. Pearson employed it extensively in the Lock Hospital, and considers it as a mode of treatment by no means applicable to general practice, though it may be advantageously employed, when it is of moment to check the disease rapidly, and where the body is so covered with venereal ulcers or eruptions, that no room is left for the ointment to be applied.[3] Dr. Colles affirms, that according to his observations, it is an erroneous opinion that ptyalism can be excited by mercurial fumigations, confined to the sound skin only.[4] Indeed, the great difficulty of preventing absorption by the lungs in these cases, the careless manner in which this circumstance is generally overlooked, and the well-known rapidity with which salivation may be thus induced, render the majority of observations already recorded of but little value in our present inquiry, and will perhaps explain those

[1] *Gibson.* Lond. Med. Repository, i. 179.
[2] *Thomson.* Mat. Med. i. 389.
[3] Ibid. i. 389.
[4] *Colles.* L. c. 58.

violent and even fatal effects which were often seen to follow the fumigating plan in the hands of the older practitioners.

Sulphur fumigations have been occasionally employed with considerable benefit, not only in cutaneous affections, but in more deeply-seated maladies. Thus, Mr. Blackett has recorded a case of sciatica, one of gout, and one of hepatic disease, cured by this treatment.[1] Hallè and Nysten have found it equally successful in rheumatism, palsy, and scrofulous tumours; and state their belief that the effects produced are not to be attributed solely to local action on the skin, but are also due to absorption of a portion of the vapours. If continued for any considerable time, it produces emaciation and general debility, even in persons of a strong habit.[2]

Rapou informs us that the vapours of narcotic and antispasmodic substances, as ether, camphor, assafœtida, ox. zinci, &c., applied to the surface of the body, will exert their peculiar actions.[3] M. Dupasquier affirms that camphor may, in this manner, be conveyed into the system, and Dr. Thomson accordingly proposes that it should be employed as an addition to the heated air, in cases of malignant cholera.[4]

Such is the sum of information which I have been enabled to collect upon this interesting question. If the evidence which it affords in favour of cutaneous absorption be not upon the whole so favourable as that obtained from the consideration of liquids, it is yet, I trust, sufficient to demonstrate that the skin, even when uninjured, is not altogether impermeable to gases.

[1] *Blackett.* Lond. Med. Rep. xx. 316.
[2] *Hallè et Nysten.* Dict. des Se. Med.—"Fumigation."
[3] *Rapou.* Dict. des Se. Med.—"Vapeurs."
[4] *Thomson,* i. 234.

DIVISION III.

ENDERMIC ABSORPTION.

The subject of Endermic Absorption, though one of great interest and importance, as affording a powerful channel through which the physician may successfully administer his remedies, and most advantageously examine their action, is yet so well known to medical men of the present day, that a very cursory notice only will suffice in this place; the immediate object of our present inquiry being to determine, as far as possible, the disputed point of absorption from the entire skin. A slight sketch, however, of its origin and progress may not be altogether useless.

The endermic method of administering remedies consists in their application to the true skin, deprived of its cuticle, by the operation of a blister or some other agent. A variety of accidental observations had been frequently made, upon the efficacy of various medicines applied to the denuded skin, before it occurred to M. Lembert that this plan might be systematically employed in the treatment of disease. Acting at once upon this suggestion, he determined to put it to the test of experiment; and the results of his observations have been laid before the world in an essay read before the Royal

Academy of Seiences, on the 25th September 1826.[1] The substanees which he has most partieularly examined are morphia, stryehnia, and quinine; but he has also made some observations on sulphuret of antimony, musk, assafœtida, saffron, tartar emetie, belladonna, squill, and corr. sublim. A eommon eantharides blister is first applied to the part, and after the discharge of the serum, the substanee, either in the form of plaster, powder, or ointment, is laid upon the denuded surfaee. The effects thus produeed are of two kinds,—the *first* local, and varying aecording to the greater or less irritating property of the drug; the *second* general, and due to absorption, not so rapidly developed, and usually accompanied with a feeling of warmth, along the eourse of the great vessels and nerves leading from the part affected to the next visceral eavity. It is a remarkable fact, and one of the greatest importance, as confirming an opinion whieh we have already laid down, in a former part of this work, when combating the arguments of those who explain every instance of absorption by previous local action, that M. Lembert has universally found a great degree of local irritation to be most unfavourable for the development of any constitutional effeets, and in this he is fully borne out by the subsequent observations of Gerhard, to be presently noticed. But to return.

The local aetion of *Morphia* is but slight, being limited to a temporary sensation of itehing; but its constitutional operation is well marked. Pain, wherever it exists, is almost instantaneously alleviated; the pupils are contraeted; there is reverie, sleep, or coma, and other head symptoms; diminished frequeney of the pulse; less bronehial exhalation; itching of the skin, and sometimes eruptions. But its great distinguishing peeuliarity, and the one whieh renders it so far superior to the internal use of opium, in many cases, is, that the stomach remains totally unaffected. The dose required to

[1] *Lembert.* Essai sur la Méthode Endermique. Reviewed in Edin. Med. and Surg. Jour. xxxi. 160.

produce full effect is about $1\frac{1}{2}$ or 2 gr., but he begins usually with $\frac{1}{16}$ gr., a large dose at first rendering the patient uncomfortably susceptible of even very small quantities. With regard to its employment in disease, M. Lembert has related twelve cases, of which the most remarkable are four of tetanus. It is unnecessary to enter into the details of these; suffice it to say, that in all, the greatest benefit was derived from the method employed.

The topical effects of *Strychnia* are still less marked; its constitutional action appears in about two hours, and in no way differs from that produced by its internal administration; and it is equally effective in the cure of disease.

Quinine has been used with great success in the treatment of ague; a considerable degree of general excitement speedily follows its application, and he finds it of importance that this excitement should correspond, as nearly as possible, with the time of accession of the fit.

The *Sulphuret of Ant.* promotes expectoration, and sometimes excites colic and diarrhœa. With *Musk* he checked a fit of asthma. *Assafœtida* and *Saffron* were useful in cases of neuralgia. *Tart. Ant.* produced violent local irritation, general diaphoresis, and diarrhœa. *Corr. Subl.*, when carefully employed, so as not to cause an eschar, excited sweating, slight increase of urine, and salivation. *Squill* acted as a diuretic; and *Belladonna* produced its usual effects, even more powerfully than when taken into the stomach.

Since the publication of these researches, a vast number of trials have been made by various observers, and most conclusive testimony has been adduced, of the great value of this method of practice. A few of these observations are noticed below, but it would occupy far more space than I can now afford, to enter fully into the subject.

The efficacy of *Morphia* thus applied has been fully confirmed by Dubourg[1] and Lesieur.[2] It has been used most

[1] *Dubourg.* Arch. Gen. x. 431. [2] *Lesieur.* Ibid. xi. 298.

successfully by Cerioli, in neuralgia and tetanus ;[1] by Meyer in hooping cough ;[2] by Blanc in rheumatism, both acute and chronic ;[3] by Ricotti in arthritis and neuralgia ;[4] by Omboni in spasmodic dysphagia ;[5] by Raciborski in obstinate vomiting and old mercurial tremors ;[6] by Magendie to relieve pain in cancer of the stomach ;[7] and by Gerhard in many diseases, but especially in rheumatism and neuralgia, accompanied with much suffering.[8] This latter gentleman, whose original paper I regret not to have been able to consult, has made a number of very careful experiments upon the method now before us. He finds both the powder and tincture of opium extremely useful, but prefers the black drop, either alone, or mixed with thick mucilage ; and of all the preparations of morphia, he gives decided preference to the sulphate. He has also employed other narcotics, as belladonna, hemlock, and datura, and finds that they act in this way precisely as when taken into the stomach.

The endermic use of *Strychnia*, though of acknowledged efficacy, has been principally confined to the treatment of paralysis and amaurosis, in which latter disease it has been employed with very various and often doubtful success.[9]

Lesieur,[10] Speranza,[11] and Chomel,[12] have each borne witness to the advantage derived from this mode of using *Quinine*, in cases of intermittent fever ; and the same plan has

[1] *Cerioli.* Arch. Gen. xx. 438.
[2] *Meyer.* Ibid. xxi. 274.
[3] *Blanc.* —— 280.
[4] *Ricotti.* —— 437.
[5] *Omboni.* —— 438.
[6] *Raciborski.* Lond. Med. Chir. Rev. July 1836.
[7] *Magendie.* Leçons sur la Vie, &c., p. 36.
[8] *Gerhard.* Arch. Gen. xxv. 420. Extracted from the North American Med. and Surg. Jour., March and July 1830.
[9] Vide Lond. Med. Gaz. vi. 479 and 543, where some cases related by *Mr Guthrie* fully bear out this assertion.
[10] *Lesieur.* Ed. Med. and Surg. Jour. xxix. 448.
[11] *Speranza.* Arch. Gen. xix. 267.
[12] *Chomel.* Lond. Med. Chir. Rev. July 1836.

been equally successful in the hands of Gerhard.[1] The absorption of the substance, in some instances, was so apparent, that it had all disappeared from the surface of the skin when the dressings were removed for a fresh application.

This observer has also made several trials with various purgative medicines, of which the most useful appears to be *Aloes*, which produces but little local irritation, and is speedily followed by copious alvine evacuations. *Gamboge*, though equally active, is more irritating, and less easily managed. *Colocynth* also acts well; but *Rhubarb* and *Jalap* are almost inert. *Croton Oil* rarely purges; and *Elaterium* only excites most violent topical inflammation. This last circumstance affords an admirable example of the truth of the assertion, that much local action is incompatible with absorption.

Of Diuretics, the principal ones which he has employed are *Squill* and *Digitalis*,—both these act with great facility. The vegetable Emetics have also considerable efficacy, but the mineral ones are too irritating. The preparations of *Mercury*, however, seem to form an exception to this general rule, being pretty rapidly absorbed. Lastly, *Iodine* applied externally, causes precisely analogous effects to those which follow its internal exhibition.

In bringing this brief and imperfect account to a conclusion, I would merely point out the very remarkable analogy which may be traced between the effects of these substances, when thus applied to the denuded skin, and those which we have already seen to follow their application to the uninjured integuments. And I would remark, that if we allow the first to be dependent upon absorption, and few will have the hardihood to gainsay this, we must of necessity attribute the same origin to the latter also, unless we are prepared at once to deny every principle of inductive reasoning.

[1] *Gerhard.* L. c. 419.

DIVISION IV.

CHAPTER I.

AGENTS BY WHICH ABSORPTION IS EFFECTED.

Of the agents by which absorption is effected, but little can be said in the present state of our knowledge. The subject is, from its very nature, involved in great obscurity, and the attempts hitherto made to clear up the mystery have been neither numerous nor satisfactory. The experiments of Schreger and of Westrumb, related above, in which certain substances applied to the skin, were afterwards found, some in the blood, and some in the lymph and chyle; the observations of Dr. Handyside, that saline matters laid on the true skin, or on granulating sores, appear exclusively in the veins;[1] and the experiment of Magendie, in which ink could be detected more easily in the subcutaneous cellular membrane than in any other part,[2]—shew at once how very uncertain are all our data, and how extremely difficult it must be to arrive at any definite conclusion. If they can be considered as proving anything, they would certainly appear to favour the opinion, that this function is carried on by simple imbibition,

[1] Vide *Alison's* Supplement, 40. [2] *Magendie.* Leçons, &c. 28.

or by a process analogous to that described by Dutrochet, under the name of endosmose and exosmose. Indeed this latter supposition is perhaps in some degree borne out by the established fact of the permeability of the skin; by the difference in density which generally exists between the fluids within the body, and those applied to the exterior, under which conditions endosmosis must of necessity take place, unless prevented by some counteracting influence; and by the power which galvanism is acknowledged to possess, of hastening this last mentioned process, while, if we may believe the assertions of M. Fabre Palaprat, it is equally efficacious in expediting cutaneous absorption.[1] It may be well, however, to remark, that such an occurrence is by no means uniform, as the following experiment will testify:—

Experiment LX.—I placed a pledget of lint soaked in a solution of hydriodate of potass on one arm, and connected it with the positive pole of a battery, while a similar pledget, attached to the negative pole, was moistened with a solution of starch, and fastened on the other arm. The application was continued until my arms were slightly vesicated, and the pain was so severe, that I could endure it no longer, but no such effects as that described by Palaprat took place, viz. the formation of iodine of starch at the negative pole, which he affirms will be observed in a few instants.

A subsequent repetition of the experiment gave precisely analogous results.

For my own part, then, although I must acknowledge that my views are by no means decided, yet from all that I have seen, read, and thought upon the subject, I am inclined to believe that the first step of the process, the first link in the chain of action, consists in endosmosis; that the matters thus introduced beyond the most external laminæ of the epidermis,

[1] *Becquerel.* Traité de l'Electricité et du Magnetisme, iv. 321, 4 tom. 8vo. Paris, 1836.

find their way partly into the sudoriferous ducts lying within its substance, and are absorbed by the vessels of their coats, (for these ducts bear every appearance of being regularly organised, and it is therefore most consistent with analogy to believe that they also possess vessels, though of so extreme a minuteness as to be invisible to the eye, even when assisted by powerful microscopes), and partly by a continuation of the original process, permeate through the entire substance of the cuticle, pass through the coats of the subjacent plexus of vessels, both lymphatic and sanguiferous, and thus reach their ultimate destination. And it should be remembered, that by subscribing to this opinion, by believing that cutaneous absorption has its rise primarily in this simple process, common alike to dead and living bodies, we do not by any means render it an exception to other kinds of absorption. The once commonly received notion of the open mouths of vessels is now rapidly disappearing before the progress of inquiry; and while it is extremely difficult to conceive that any substances can enter vessels thus entirely closed, unless by a species of transudation through their coats, the experiments of Magendie have unequivocally shewn that such transudation may take place, even through the dense parietes of a large artery; and it cannot therefore be supposed that the thin and almost imperceptible walls of their capillary terminations will offer any material obstruction. The horizontal position of the cuticular cells, and the vertical direction of those which exist in the epithelium of the intestines, may perhaps afford some explanation of the different degrees of rapidity with which the skin and mucous membranes absorb, since it is at once evident that the facilities for permeation are, in the one case, incomparably greater than in the other.

CHAPTER II.

APPLICATION TO THERAPEUTICS.

We have already treated this subject so fully, when considering the evidence of absorption afforded by the employment of medicinal agents, that but little now remains to be done beyond considering the peculiar advantages held out by this system, and the particular cases to which it is most applicable. The mucous membrane of the alimentary canal presents so extensive a surface, one so admirably fitted for absorption, and so closely connected with every part of the body, through the medium of sympathy, that it must evidently afford, in the majority of cases, by far the most eligible channel for the exhibition of our remedies. And it will appear still more appropriate, if we reflect upon the number of diseases which are more or less dependent upon an altered state of its secretions, or a derangement of its other functions; cases of daily occurrence, and in which the immediate application of the counteracting agent to the part affected, is certainly preferable to every other method. But there are also conditions, and these by no means uncommon, under which this plan of treatment would be injudicious, or even impossible; and it is in these cases that the greatest benefit may be expected from the introduction of medicines through the skin. Thus, when the stomach is so irritable, that the smallest quantity of ingesta causes immediate vomiting, no method has been found more useful to remove this state than the endermic use of morphia.

When, under the same circumstances, it is of material importance that the small as well as the large intestines should be evacuated, the external application of purgatives will effect this purpose. Or should it be necessary to bring the patient under the influence of mercury, to excite diuresis, or relieve concomitant spasmodic affections, all these indications may be speedily and effectually fulfilled by the same means. In cases of dysphagia, whether of a temporary or permanent nature, the great superiority of this method is sufficiently evident. It is of no less value in the treatment of the diseases of children, and in adults may be advantageously employed, when there is an invincible repugnance to the remedy, or when, from long-continued use, the stomach has become insensible to its impressions; or again, when its internal administration is accompanied with disagreeable effects. Thus Chrestien informs us that he has known many patients who could not bear the internal exhibition of corr. subl., but who were much benefited by its external application.[1] Every practitioner must have met with instances of violent irritation succeeding to the internal use of iodine, and have been annoyed by derangement of the digestive organs, from the employment of opium. In each and all of these cases, the same remedies may be administered with the greatest advantage, through the medium of the common integuments.

But if the method we are advocating be thus useful in the treatment of general disease, it is still more worthy of confidence in the management of local affections. We have seen in neuralgia, how greatly the patient's torture may be alleviated by anodyne applications, and we have even reason to believe that the malady may be thus entirely removed. We have brought forward abundant evidence to prove the value of this plan, in combating local inflammations; and the experience of every surgeon will suggest fresh cases to his mind. And we have seen that it affords equal, if not greater, pro-

[1] *Chrestien.* Méthode Iatraleptique, &c. note, p. 14.

spects of success than the more usual method, in the removal of those chronic enlargements of various organs, which so often try the patience, and exhaust the resources, of the medical attendant. Indeed, a little reflection will shew that such might have been expected from it *a priori*. Without entering into any unprofitable speculations regarding the proximate cause of disease, it may be affirmed, without much fear of contradiction, that a great proportion are connected with some change of action, either in the capillary vessels, or in the nervous ramifications; and such being allowed, it is at once self-evident, that we shall have more chance of altering this condition, and of bringing it back to the healthy standard, by the application of our remedies immediately to the part affected. Hence, where the alimentary canal is involved, the internal use of medicines, unless contraindicated, or prevented by the circumstances above noticed, is decidedly preferable; but where the disease lies nearer the surface, the skin presents a channel the most direct and eligible for our purpose.

There is yet another advantage which this plan possesses, of great value to the physician, though less important to the patient, viz., the facility which it affords of studying the action of his remedies, uncomplicated and unobscured by those sources of fallacy which so frequently attend their internal exhibition. A medicinal agent, when introduced into the stomach, becomes mixed with its healthy or diseased secretions; it undergoes in many cases a species of digestion, and is then carried into the circulation, with properties greatly modified, or perhaps totally altered. No such change can be effected at the skin. In the former case, the surface to which it is applied is frequently diseased; in the latter, a healthy portion may be always chosen. In the former, the local irritation may affect the whole system, while in the latter such an accident can rarely occur.

But there are two methods by which this end may be accomplished; to which of these should we give the prefer-

ence? When it is desirable to bring the system rapidly under the influence of our remedy; when the substance is expensive, or can act in very small quantities, and when the local irritation produced by the means taken to denude the skin would not be injurious, the endermic plan would decidedly merit our preference. But when a slower effect is all that we require; when the medicine is less costly, and may be used in larger quantities; when it is desirable to avoid every local excitement; and when the remedy to be employed is either in the liquid or the gaseous form, it will be most advantageous to apply it to the unbroken surface. In children especially, this latter plan is infinitely superior, both as being more easily effected, and unattended by any pain.

But while I would thus strongly advocate the method now before us, I am not blind to its disadvantages. I allow that it is perhaps less certain, and assuredly more tedious, than the ordinary mode. I allow that it would be not only absurd, but unwarrantable, to trust to it alone, in many cases; and that numerous complaints will be far better treated through the medium of the stomach and intestines. But I must maintain that it possesses some advantages of which no other plan can boast, and that by discarding it altogether, we should rob ourselves of a most useful and important auxiliary.

It only remains, therefore, before bringing my labours to a close, that I should add a few words by way of resumé. In the foregoing pages, we have traced the remarkable analogy presented by the tegumentary organs throughout the animal kingdom, in all its gradations, from the highest to the lowest. We have seen that, while varying greatly in degree and extent of development, they have yet, in accordance with the beautiful and uniform simplicity of nature, been all, or almost all, constructed upon the same general plan; and we have hence argued, that since cutaneous absorption is undoubted in the lower tribes, we might fairly assume that it is not altogether wanting in the higher. Proceeding still farther, we have shewn, in opposition to the assertions of many phy-

siologists, that the human epidermis, in place of being a mere inorganic varnish, spread over the surface of the true skin, to protect it from injury, and obstruct the passage of deleterious agents, is a membrane of a somewhat complex structure. We have seen that even if it does not possess a set of vessels peculiar to itself, it has yet embedded in its substance loops of the subjacent plexus of lymphatics, this arrangement being doubtless adopted for some specific purpose. We have shewn that it is not impermeable to fluids, and we have advanced reasons for distrusting the opinions of those who hold it to be destitute of vitality.

Fortified by these anatomical preliminaries, we have gone forward in our inquiry. We have found that various parts of the body, when steeped in fluids of a bland unirritating nature, attract to themselves an appreciable quantity; and that the rate of this imbibition may, to a certain extent, be determined by actual admeasurement. We have observed the *absolute* increase of weight which follows immersion in the warm bath, when no possibility of pulmonary absorption could exist, and at the same time we have endeavoured to prove, that the danger of fallacy from this source has been greatly exaggerated. We have seen thirst removed by the same means, and have reviewed those singular instances of long-continued abstinence from drink, of excessive and prolonged discharges, and of suddenly augmented weight, which can be explained upon one supposition only, viz., absorption from the atmosphere. We have next drawn arguments from the introduction of malaria into the system; we have proved the absorption of putrid exhalations, and shown how strongly the doctrine of contagion supports our proposition.

Nor does our evidence end here. By numerous and diversified observations, and by unequivocal experiments, we have demonstrated the important fact, that medicinal agents, when applied to the uninjured skin, be their form what it may, be they solid, liquid, gaseous, or in vapour, will speedily reach the interior of the body, and there exercise their accustomed

actions. In this manner we have seen pain alleviated, and disease removed. We have seen death produced by the external application of a poison. Nay more, we have followed a substance thus placed in its progress inwards; we have detected its peculiar odour in the secretions; and, finally, we have shewn its presence, by the almost unerring test of chemical analysis.

Can more than this be required? Surely even the most sceptical, after an impartial consideration of these proofs, can scarcely persist in refusing his assent to the existence of Cutaneous Absorption. Unless utterly immoveable by testimony, he must at length be compelled to acknowledge that, if all this be true, the evidence is overwhelming; that the doctrine rests upon as sure a basis as we can ever hope to obtain in physiological inquiries.

THE END.

EXPLANATION OF THE PLATE.

Fig. 1. Represents a vertical section of a small piece of human cuticle, separated from the cutis.

a, *a*. Sudoriferous canals.

b, *b*. Filaments resembling the vessels described by Breschet.

c, *c*, *c*, *c*. Internal layers of the cuticle, softer and more opaque than the external.

Fig. 2. Cells from the internal surface of the cuticle of the penis.

Fig. 3. Cells from the same part, drawn from the camera lucida.

Fig. 4. Scales or cells from the mucous membrane of the mouth.

Fig. 5. Ditto from the epithelium lining the superior part of the œsophagus of a cat. (*Henle.*)

Fig. 6. Ditto from the internal surface of the conjunctival epithelium of man. (*Henle.*)

Fig. 7. Cylinders from the cardiac region of the human stomach. (*Henle.*)

Fig. 8. Ditto from the human jejunum.

Fig. 9. Ditto from the human gall bladder, shewing the manner in which they are united. (*Henle.*)

Fig. 10. Ditto from the human jejunum, viewed from their free surface, and exhibiting the fine net-work which forms their bond of union. (*Henle.*)

Fig. 11. Transverse section of a villus, shewing how the sheath of epithelium is formed. (*Henle.*)

Fig. 12. Vibratory cylinders from the trachea of a cat, viewed immediately after death. (*Henle.*)

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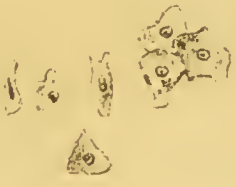

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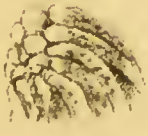

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W. H. M.